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REACTION-TIMES OF CHILDREN

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There is ample evidence to show that certain innate abilities, such as intelligence and memory, improve with age. Studies such as those of Gesell[1] also indicate that some motor abilities develop even when there has been no training, so that maturation is a factor in learning. It would therefore seem of interest to determine the development with increasing age of a motor ability not previously learnt. As most motor abilities are utilized in some form or other almost constantly throughout life one would expect that there would be some transfer to the performance of a supposedly new motor task. The amount transferred in this fashion is hardly subject to experimental investigation: consequently one selects an ability which appears to involve to a minimum extent transfer from kindred habits already formed.

Such an ability seems to be that involved in reaction-time. Certainly there is little or no previous practice of reaction-time at least in the form used in the psychological laboratory. It is true that all throughout life individuals are constantly being called upon to act with the utmost celerity, but generally the act involves the greater part of the musculature and is not as definitely localized in a few muscles as is reaction-time. Moreover, reaction-time has certain advantages over most motor skills that render it suitable for investigation: it is adapted to the entire age range; its simplicity makes it ideal for younger children, yet the concentration required to act with maximum quickness challenges the most mature subjects; it can be easily and quickly measured in exact units.

As there are various stimuli to which one can react, and as reaction-time

* Accepted for publication April 18, 1932.
[1] A. L. Gesell, Maturation and infant behavior pattern, *Psychol. Rev.*, 36, 1929, 307-319.

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may or may not have a preparatory signal—types of reaction which seem to be closely similar—opportunity is thus offered to determine the correlation between cognate motor abilities. In addition there is the possibility of finding the relationship between intelligence such as measured by the ordinary verbal group tests and the motor ability of reaction-time. Finally the data collected for these purposes enable one to test on a more elaborate scale than has hitherto been possible the criterion devised by Woodrow to measure attention.[2]

The following specific objects were held in view in this investigation: (a) How does reaction-time vary with age in school children? (b) What is the relationship between various types of reaction-time? (c) How is intelligence related to reaction-time? (d) Does Woodrow's method of reaction-time difference measure attention?

Procedure

Subjects. The *Ss* for this experiment were all the children from Grades IV to VIII inclusive of Grandin Separate School, Edmonton. To yield an adequate number of younger *Ss*, 16 girls and 20 boys from Grade III were included in the group and their reactions were taken in the month of April, some 5 mo. previous to their entrance into Grade IV. A few children over 16 yr. and 6 mo. were not included in the tabulations. A total of 311 *Ss*, 165 girls and 146 boys, took all the tests. They were distributed by age as follows:

Age	9 yr.	10 yr.	11 yr.	12 yr.	13 yr.	14 yr.	15 yr.	16 yr.	Total
Boys	22	22	22	19	18	22	14	7	146
Girls	20	18	25	31	28	25	12	6	165

Age 10, throughout this paper, means from 9 yr. 6 mo. to 10 yr. 6 mo.

The experimental work was begun in October, 1930, and continued till the end of April, 1931.

Apparatus. The apparatus used was a commercial set-up called a psychodometer, the timing element of which consists essentially of a Dunlap chronoscope synchronized by a 50 d.v. tuning fork. *S* sat comfortably before the reactor's board in a quiet room. Before him were the reactor's key—a telegraph sounder—and the light stimulus—a 6-volt flashlight bulb shining through a frosted window. Twenty feet away, so that the noise of the instruments would not disturb *S*, sat *E* before a table on which were the chronoscope, the tuning fork, the master key, and the stimulus-keys. *E* could see *S* from the side so as to be sure

[2] This method has been used by the following investigators: H. Woodrow, The measurement of attention, *Psychol. Monog.*, 16, 1914, (no. 76), 1-158. About 20,000 reactions from 12 subjects—7 adults and 5 children. B. R. Philip, The measurement of attention, *Stud. Psychol. & Psychiat.*, 1, 1928, (no. 2), 1-65. About 7800 reactions from 61 college students. H. Easley, An attempt to isolate the factor of attention, this Journal, 43, 1931, 202-215. About 6000 reactions from 68 college students. The present paper is a study of about 40,000 reactions from 311 children.

he was reacting properly; a precaution which generally was necessary only while the *S* was taking practice reactions.

As the sounder was about 60 cm. away from the children it was not thought necessary to make any correction for velocity of sound. Neither was any correction made for delay in the light bulb coming to incandescence, as the lag in the bulb as measured on an oscillograph was approximately 2 σ, hence of the same approximate value as the time required for the sound to reach *S*. The lag in the instruments was not investigated; however, for instruments of this type it is small and fairly constant.

Method. All the reactions were taken in the afternoon from 1.30–4.00. The children came from their class in pairs. They were shown how to place their hand on the key, using the first and second digits only, while the forearm rested comfortably on the table, and how to lift their hand quickly when the sound or the light stimulus was perceived. The instructions were simple:

Place your hand on this key as follows. I will say "Ready" as a warning. Shortly after the sounder will give the signal to act. When you hear the sound, lift your hand as quickly as you possibly can, and then place it on the key for another trial. Remember you must lift your hand the very instant you hear the sound.

The sound stimulus was then given several times so that the children could readily identify it, and from 10–12 reactions were taken but were not recorded. During this trial period *E* watched *S* so as to be sure that he was reacting properly. Now and then he would encourage him to react as quickly as possible. When certain that the reactions were being properly made the regular series was begun.

The reactions were taken in the following order: (1) sound with warning; (2) sound without warning; (3) light with warning; (4) light without warning. Thirty reactions of each kind were recorded; they were divided into 3 groups of 10 each. A short rest of about ½ min. was given between each group of 10 reactions, while *E* checked his tenth reading and reset the dial of the chronoscope to zero. During this period the child was told to take his hand off the key and to rest.

When the first group, the reactions to sound with warning, had been taken the second child who had been watching his companion had his reactions to sound with warning taken; meanwhile the first *S* rested. Thus they alternately reacted and rested. The rest periods were quite important as the children easily became fatigued. They had a tendency to cramp their hand too tightly on the key despite the warnings of *E*, and some few when questioned complained of cramped fingers. Even though each child had a short rest of about 30 sec. after every 10 reactions and a rest of from 5–10 min. between different groups of reactions, a slight fatigue effect was apparent which will be referred to later.

Two experimenters took all the reactions, the writer and Mr. Aiello, a graduate student in psychology. Both *Es* had practiced together for 6 experimental periods so that their methods of giving the reactions were very similar and they had learned to read the movements of the dial to interpret any unusual reaction. For all practical purposes their technique was identical.

The instructions when no warning signal was given were:

This time I will not say "Ready"—you must be ready all the time. Remember as soon as you hear the sound [see the light] lift your hand as quickly as you possibly can from the key.

Before every group of reactions the instructions were carefully explained, and a practice series of from 10–12 reactions were given. No *S* was allowed to proceed till he had performed his task thoroughly well. In general there was little trouble except among some of the younger children who were prone to react prematurely. A premature reaction either did not record owing to the wiring arrangement, or if the reaction closely synchronized with the stimulus to react a peculiar floating effect of the dial indicator was observed. In the event of an incorrect reaction or one that was obviously improperly made no record was taken.

The warning signal for both sound and light was the quietly spoken command "Ready" preceding the stimulus from 1–2 sec. Care was taken to vary the warning interval within these limits. When the reactions with warning were given the reactions were taken almost as fast as they could be read and recorded, except for the rest between each group of 10 reactions. When no warning signal was given the reactions were taken much more slowly, the fastest one being given about 6 seconds after the preceding one, and the slowest ones about 20 seconds after the preceding one; the average interval for the reactions was about 10 seconds. In the no warning group care was taken to vary the reaction interval as much as possible.

Variables. The following variables were chosen: sound with warning (SW), sound unprepared, *i.e.* without warning (SU), light with warning (LW), light without warning (LU), the difference between the first pair, called sound difference (SD), and the difference between the second pair, called light difference (LD). The last two variables were to be compared with Woodrow's criterion of attention, which is the reciprocal of the difference between the reactions with warning, and those without warning. Scores on the Otis Intelligence Test were also tabulated (MA).

Results

Practice and fatigue. To ascertain the effects of practice and fatigue the results of the first two classes which took the reactions were used. There were in this group 82 *S*s (36 boys and 46 girls) all of Grade V standing except 10 boys and 10 girls from Grade VI. The average age of the boys was 11 yr. and 4 mo.; of the girls, 11 yr. and 6 mo. The reactions of each of the 4 types were divided into 3 groups of 10; thus the average of the first 10, the middle 10, and the last 10 for each *S* were separately recorded and averaged. A comparison of the 3 average values would enable one to tell whether practice or fatigue had most effect on the score. By practice is here meant any change in *S*'s reactions which would tend to diminish the reaction-time; by fatigue, any change in *S*'s reaction which would tend to increase his reaction-time. The results are given in Table I; the reactions for boys and for girls are separately tabulated, and the values for all the children are given.

A survey of the table shows that the fatigue effect is greatest in the cases of sound and light with warning, there being a retardation of 11.0 σ and

12.1 σ respectively in the base of the boys, and 8.6 σ and 7.6 σ in the case of the girls, when the averages of the fastest and the slowest groups of 10 are taken. The retardations for sound and light without warning, both for the boys and the girls, are practically the same and amount to about 6 or 7 σ.

TABLE I

AVERAGE REACTION-TIME IN SIGMA FOR BOYS AND GIRLS AND FOR ALL Ss FOR THE DIFFERENT GROUPS OF 10 REACTIONS, TOGETHER WITH THE GROUP-VARIATION FROM THE TOTAL AVERAGES

Condition	Group	Boys (N=36)		Girls (N=46)		Total (N=82)	
		σ	Var.	σ	Var.	σ	Var.
Sound	1st. 10	171.3	−6.8	177.0	−6.2	177.44	−3.46
with	2nd. 10	180.5	2.4	187.0	3.8	184.18	3.28
warning	3rd. 10	182.3	4.2	185.6	2.4	184.34	3.44
	All	178.1	*(11.0)	183.2	(8.6)	180.90	(6.90)
Sound	1st. 10	199.3	−2.5	202.6	.3	201.16	− .87
without	2nd. 10	200.3	−1.5	199.5	−2.8	199.88	−2.15
warning	3rd. 10	205.2	3.4	205.3	3.1	205.27	3.24
	All	201.8	*(5.9)	202.3	(5.9)	202.03	(5.39)
Light	1st. 10	190.5	−8.4	184.8	−3.5	187.26	−5.66
with	2nd. 10	203.6	4.7	187.8	− .5	194.74	1.82
warning	3rd. 10	202.6	3.7	192.4	4.1	196.84	3.92
	All	198.9	*(12.1)	188.3	(7.6)	192.92	(9.58)
Light	1st. 10	237.8	−3.8	229.1	−4.6	232.98	−4.19
without	2nd. 10	242.7	1.1	235.9	2.2	238.95	1.78
warning	3rd. 10	244.3	2.7	235.9	2.2	239.61	2.44
	All	241.6	*(6.5)	233.7	(6.8)	237.17	(6.63)

* Numbers in brackets are the extreme ranges of average reactions in group.

Despite the obviously greater difficulty in maintaining attention when no warning signal is given and the interval is prolonged up to 20 sec., what we have been calling fatigue is not as manifest as in the case when a warning is given. Nevertheless the children showed greater signs of fatigue and when questioned admitted the unprepared reactions were more tiring than the warning ones. The explanation of this anomaly probably lies in the fact that the giving of a warning signal aroused a tendency to anticipate the stimulus, with a consequent unreadiness to react when the actual stimulus came. This anticipatory tendency is built up during the first 10 reactions and thus would be more manifest in the second and third 10's than in the first 10 of the group of 30 reactions.

As a rule the first group of 10 is the only one faster than the mean reaction of the 30; the exceptions are the sound without warning, SU for the

boys, and sound without warning, SU, and light with warning, LW, for the girls. In these instances, possibly a tendency to perseveration or inadaptability to the new situation makes itself manifest. Thus in the case of the SU reactions, the *Ss* were not thoroughly adapted to the absence of the preparatory signal and consequently their first 10 reactions were slower than

TABLE II

MEAN REACTION-TIME IN SIGMA

	Age	N	SW	SU	D	LW	LU	D
	9	22	172.94	222.84	49.90	193.50	260.08	66.58
	10	22	172.20	210.86	38.66	186.38	235.94	49.56
	11	22	170.90	197.42	26.52	190.40	234.06	43.66
	12	19	167.28	196.66	29.38	189.82	231.32	41.50
Boys	13	18	160.76	180.34	19.56	180.02	214.96	34.94
	14	22	156.18	174.66	18.50	173.36	203.96	30.60
	15	14	139.50	151.14	11.64	154.82	173.62	18.82
	16	6	136.12	148.46	12.34	145.58	169.66	24.06
	9	20	180.96	215.00	34.02	195.06	241.72	46.66
	10	18	179.82	205.20	25.40	183.50	229.58	46.08
	11	25	167.42	211.98	34.56	192.67	241.68	48.98
	12	30	173.82	201.02	27.20	181.42	228.32	46.90
Girls	13	28	162.72	194.24	31.54	176.72	224.58	48.03
	14	25	153.60	188.10	34.48	165.56	218.26	34.62
	15	12	150.42	181.40	31.16	162.20	212.16	50.00
	16	6	142.90	197.44	54.54	170.60	240.40	69.80
	9	42	176.76	219.10	42.34	194.24	251.34	57.10
	10	40	175.64	208.32	32.68	185.08	233.08	48.00
	11	47	174.38	205.18	30.78	191.62	238.12	46.48
	12	49	171.28	199.32	28.06	184.68	229.50	44.82
All	13	46	161.96	188.80	26.84	178.02	220.94	42.92
	14	47	154.76	181.80	27.00	169.22	211.14	42.22
	15	26	144.54	165.12	20.64	198.20	191.26	33.16
	16	12	139.52	172.96	33.44	158.04	205.04	46.94

they normally would have been. If such is the correct explanation in general the boys were better able than the girls to adapt themselves to the new situations. A comparison of the means for the boys and the girls gives us an indication of a finding that shall later be discussed more thoroughly. The boys of this group are some 5 σ faster for sound with warning than the

TABLE III

AGE NORMS FOR REACTION-TIME: SMOOTHED MEANS

Age	N	SW	SU	SD	LW	LU	LD
9	42	176.76	219.10	42.34	194.24	251.34	57.10
10	40	174.92	210.86	35.26	192.32	240.84	50.52
11	47	173.76	204.28	30.50	187.12	233.56	46.44
12	50	169.20	197.76	28.56	184.78	229.46	44.74
13	46	162.66	189.98	27.30	177.30	220.52	43.32
14	47	153.76	178.58	24.82	168.48	207.98	39.44
15	26	144.54	165.12	20.64	158.20	191.36	33.16

girls; the groups are approximately equal for SU; for both reactions to light the girls are almost 10 σ faster than the boys.

Age norms and statistical values. The age norms for the variables chosen

TABLE IV

VARIATION OF REACTION-TIME WITH AGE AS PERCENTAGES OF THE 12-YR. NORMS

Age	N	SW	SU	SD	LW	LU	LD
9	42	105.5	110.7	148.2	105.3	109.6	127.7
10	40	103.5	106.8	123.7	103.2	105.1	108.0
11	47	102.8	103.6	107.0	101.4	101.8	104.0
12	50	100.0	100.0	100.0	100.0	100.0	100.0
13	46	96.1	96.0	95.6	96.1	96.1	96.8
14	47	91.0	90.3	87.1	91.2	90.5	88.2
15	26	85.5	83.7	72.3	85.7	83.3	74.0

are given for girls, boys, and all *Ss* in Tables II and III. It is to be noted that there is a consistent increase in duration from sound and light with warning, to sound and light without warning. In general the tables show the following characteristics: the reaction-times of all the variables except SW show a perceptible drop at age 10 yr. followed by a rise at age 11 yr. This variation can only be accounted for on the basis of inadequate sam-

TABLE V

MEAN SCORE: PERCENTAGE OF 12 YR. NORMS

	Age	SW	SU	SD	LW	LU	LD
	9	103.2	113.5	170.0	102.0	112.8	160.5
	10	103.0	107.2	131.7	98.2	102.0	119.5
	11	102.0	100.5	90.3	100.5	101.4	105.6
	12	100.0	100.0	100.0	100.0	100.0	100.0
Boys	13	96.0	91.7	66.6	95.0	93.0	80.5
	14	93.4	89.0	63.1	91.5	88.1	70.5
	15	83.5	77.0	39.6	81.5	75.0	43.6
	16	81.5	75.6	42.0	76.8	73.3	55.4
	9	105.1	107.1	125.1	107.3	105.7	99.4
	10	103.2	102.5	93.5	101.1	100.5	98.3
	11	102.0	105.9	127.0	106.1	105.6	104.4
	12	100.0	100.0	100.0	100.0	100.0	100.0
Girls	13	93.6	97.0	116.0	97.3	99.3	102.2
	14	88.4	93.9	127.0	91.3	95.7	116.5
	15	88.6	90.0	114.3	89.5	92.6	106.7
	16	82.3	98.0	201.0	94.0	105.3	148.4
	9	103.1	109.6	150.9	105.1	109.8	127.3
	10	102.8	104.2	116.4	100.2	101.8	107.1
	11	101.8	102.6	109.5	104.0	104.0	104.0
	12	100.0	100.0	100.0	100.0	100.0	100.0
All	13	94.6	94.4	95.7	96.7	96.6	95.7
	14	90.3	90.9	96.2	91.7	92.2	94.1
	15	84.4	82.6	73.5	85.8	83.3	73.9
	16	81.5	86.5	119.2	85.6	89.5	104.6

TABLE VI

RELIABILITY OF THE DIFFERENCES

	Girls			Boys			Diff.	S.D.diff.	D/S.D.diff.	Chances in 100
	Mean	S.D.dis.	S.D.M	Mean	S.D.dis.	S.D.M				
SW	168.28	26.01	2.03	162.60	23.96	1.98	5.68	2.83	2.00	98.0
SU	201.20	30.94	2.42	191.10	35.96	2.96	10.10	3.83	2.64	99.6
SD	32.33	25.08	1.95	28.42	26.52	2.13	3.92	2.89	1.35	91.0
LW	180.01	26.66	2.08	180.96	23.76	1.97	−0.96	2.86	−0.34	64.0
LU	228.88	32.48	2.53	222.40	40.84	3.39	6.50	4.23	1.54	93.4
LD	49.78	24.78	1.93	41.38	29.93	2.47	8.40	3.14	2.68	99.7
MA	34.92	16.00	1.25	34.05	15.65	1.30	0.87	1.80	0.48	68.0

TABLE VII

EFFECT OF ADOLESCENCE

		Age	SW	SU	SD	LW	LU	LD	MA
Girls	Adolescents	14.11	154.55	197.56	43.01	173.22	232.01	58.79	44.9
	Non-adol.	13.8	150.20	182.49	32.28	163.19	211.86	48.66	51.4
	Diff.	1.3	4.35	15.07	10.73	10.03	20.15	10.13	−6.5
Boys	Adolescents	14.8	145.28	158.68	13.38	159.56	181.58	22.02	49.24
	Non-adol.	13.6	146.71	159.84	13.13	164.31	186.51	22.20	49.28
	Diff.	1.2	−1.43	−1.16	0.25	−4.75	−4.93	−0.18	−0.04

pling. A second characteristic is the rather rapid rise for the girls at 15 yr. of age. It will be shown later that this is probably accounted for by the retardation of reaction-time for some girls at this period due to adolescence. This effect is not noted in boys at the corresponding age.

Possibly the varying rate of reaction-time (RT) can be seen most clearly by calculating the percentages of all the age norms in terms of the 12-yr. means. They are given in Tables IV and V. Reaction-time differences for

TABLE VIII

CORRELATION TABLES

		SW	SU	SD	LW	LU	LD	MA
	SU	.625						
	SD	−.243	.597					
Girls	LW	.700	.644	.101				
(N=165)	LU	.523	.796	.468	.655			
	LD	−.078	.418	.524	−.188	.604		
	MA	.115	.251	.008	.236	.196	.011	
	CA	−.448	−.297	.095	−.376	−.206	.177	.523
	SU	.734						
	SD	.056	.738					
Boys	LW	.735	.680	.178				
(N=146)	LU	.629	.834	.577	.680			
	LD	.224	.590	.618	.028	.787		
	MA	.348	.364	.178	.415	.356	.228	
	CA	−.451	−.610	−.408	−.562	−.622	−.420	.605
	SU	.684						
	SD	−.095	.669					
All	LW	.706	.642	.134				
N=311	LU	.569	.817	.527	.656			
	LD	.087	.525	.575	−.088	.711		
	MA	.214	.300	.088	.332	.274	.113	
	CA	−.438	−.463	−.160	−.457	−.435	−.154	.584

sound and light show the greatest deceleration, then sound and light without warning, and least of all, sound and light with warning. For SW and LW the deceleration amounts to about 5% from year 9 to year 12, and from year 12 to year 15 it amounts to about 14%. Thus the deceleration in the 3-yr. period 9-12 is only about one third of the deceleration in the following 3-yr. period. Sound and light without warning (SU and LU) show decelerations of about 10% and 16% in these two triennial periods; while SD and LD are more closely alike in the two triennial periods and amount to about 30%.

An item of no little interest is the fact that the decelerations for sound and light are very closely the same whether one considers the reactions with preparatory signal or without preparatory signal, and there is but slight variation in the case of RT differences for sound and light. It is evident

that the psychological factors involved are closely similar when sound and light are the stimuli, but that they vary with the mode of giving the stimuli. Corroboration of this fact is found in Table VIII, which gives the correlation of MA with each of the variables mentioned. The values for sound and light are approximately the same though this value common to sound and to light differs with the mode of giving the stimuli and with sex.

Frequency distributions of the variables. Table XI gives the constants

TABLE IX

DECREASE IN REACTION-TIME FROM 9–15 YR. OF AGE

Sex	Age	N	SW	SU	SD	LW	LU	LD
Boys	9	22	172.94	222.84	49.90	193.50	260.08	66.58
	15	14	139.50	151.14	11.64	154.82	173.62	18.82
	Diff.		33.44	71.70	38.26	38.68	86.46	47.76
Girls	9	20	180.96	215.00	34.02	195.06	241.72	46.66
	15	12	150.42	181.40	31.16	162.20	212.16	50.00
	Diff.		30.54	33.60	2.86	32.86	29.56	−3.34

of the frequency distributions for all the variables. The curves were plotted but are not published here. The means and the standard deviations are tabulated as are the constants of the normal curves of best fit. Several facts are worthy of note: there is a tendency for the boys' curves to be flattened, with a consequency greater standard deviation, and the contrary tendency for the girls' distributions to be more frequent at the mean. The boys' curves are skewed positively, that is with increased frequencies toward the smaller scores; hence the boys' scores are in general lower than those of the girls. Later we shall see that there is for some of the variables a statistically significant difference between the sexes. Some of the distributions approximate the normal curve of error so closely as to enable one to use with some degree of confidence statistical devices based upon the probability curve. The curves for light are particularly close to the normal curve. The curves for sound have a tendency to bi-modality due to the significantly faster times of the boys. Some of the values of the RT difference curves, both for sound and for light, are negative. An explanation of these negative values in terms of Woodrow's theory of detractors would be difficult; this will be discussed in another section.

Sex differences. Table VI gives the reliability of the differences between the girls and the boys for the variables. It will be seen that there is a slight, though fairly significant difference between sexes for all the variables except LW and MA. The significance is expressed in terms of the number of chances in 100 that the difference is a true difference. The difference is always in favor of the boys, that is they react faster than do the girls; it

is of the order of 3-5% to reactions with and without warning, 20% for RT differences to light (LD), and 14% for SD. These sex differences are calculated from all the scores of the boys and of the girls. In Table I, which gives for Grade V the effects of fatigue and practice, the boys are 2.8% faster than the girls in the case of SW; there is little or no difference for SU. For both the light reactions the girls are faster than the boys, 5.2% for LW and 3.3% for LU. As will be seen in the next section adolescent girls tend to slow up in their reactions though the boys do not; hence when all the children are considered the boys are definitely faster than the girls except for LW.

The differences are so slight that they have hitherto been but little noticed, due to the masking effect of individual variability. They need not necessarily be explained as due to innate sex differences; it may be that the motor training of boys, particularly from play, tends to develop faster reactions to sound than that of the girls whose games do not have that tendency. A similar explanation would account for the fact that the younger girls are faster in light reactions than are the boys. Such an explanation is of course but tentative and conjectural.[3]

Reaction-time and maturity. Reaction-time being a special type of skill in which the children are unpracticed, all improvement from year to year, save that obtained by transfer from motor activity generally, is the result of maturity. The decrease of RT with age gives us definite evidence that the increased coördination brought about by time is not confined to infancy and to the early years of childhood. It would have been a matter of some interest to obtain the variation of RT with age up to full maturity. As this study was confined to grade children too few *S*s were available beyond 15 yr. of age. Growth in this ability shows no sign of diminution up to that period, as the yearly percentage decrements were increasing from ages 12–15 yr.

There is however a most interesting effect to be noticed at the older ages studied which shows for girls but not for boys a definite slowing up of reaction-time. Table VII brings this out clearly. The tabulations were obtained as follows. The children in Grades VII and VIII were divided into adolescents and non-adolescents by their teachers. In the group chosen there were 20 adolescent girls and 29 adolescent boys, while 38 girls and 18 boys were non-adolescent. The criterion used to determine adolescence was a subjective one, and to that extent the inferences based on the data are

[3] The slowing up of the adolescent girls' reactions which is discussed in the next section, increases the mean scores of the girls. This factor would, of course, account for some of the difference but not for all.

invalidated, but as a first approximation the criterion was the best that could be obtained in the circumstances. Though the adolescent girls were on the average 1 yr. 3 mo. older than the non-adolescent girls and hence should have quicker reactions there is a definite retardation for them for every type of reaction varying from 3% in the case of SW to 9% for LU, and from 17% for LD to 25% for SD. The reactions of the adolescent boys are slightly faster than those of the non-adolescent. One would expect this difference to be due chiefly to the fact that the adolescent boys are on the average 1 yr. 2 mo. older than the rest of the group.

Had a study been made of High School students, where larger numbers of adolescents would be available and where the age range includes *S*s at various stages of adolescence, and had objective criteria of adolescence been used, one would be in a better position to state whether the tendency here indicated had a factual basis or be but the chance result of insufficient data and uncertain criteria.

Correlation of abilities. An inspection of Table VIII reveals the fact that the correlations may be roughly divided into two classes: those that are positive and quite definite, ranging from 0.42 to 0.83, and those that are negative and weak, though they may still be significant. In this latter group are the four correlations SD-LW, SW-LD, LW-LD, SW-SD, which show the relationship between reactions with warning and reaction-time difference. Evidently these abilities are not closely allied—which is pretty much what one would expect for the one implies ability to release nervous energy with facility, while the other implies ability to hold it at constant intensity during small time intervals.

When one compares the correlations for the girls with those of the boys it is found that the correlations of the girls range from —0.243 to 0.796, while those of the boys range from 0.028 to 0.834. Hence there is not a variable to variable parity between the correlations, those of the girls are characterized by a wider range and by negative values in some cases. When, however, the correlations of the girls and those of the boys are tabulated for the same variables, and themselves correlated, the Pearson r found is quite high, ($r = 0.963 \pm .01$). This result is based upon 21 variables including those of MA.[4] If we leave out the intelligence variables the value of r still remains about 0.96. Thus while the individual RT of an *S* shows a variability which is more than 10% of the mean, and while the standard deviations of the distributions of those means are themselves rather high,

[4] The sign of the MA correlations was changed to obtain values for the regression line, $Y = 1.163\ x\ —2.06$. The change of sign does not alter the conclusions, as the amount, not the direction, of the correlations was required.

yet the functions we are considering are so related that a very definite agreement exists between the two sets of correlations.

It is to be noted that the girls' correlations are not as high as those of the boys. If we disregard sign, which indicates the direction of the relationship not its amount, the average correlation, excluding those with MA, is, for the girls 0.421, and for the boys 0.501. A partial explanation for this fact is that the girls are not as variable as are the boys and tend to the mean more than do the boys; also the effect of adolescence on the girls and not on the boys is to limit quicker reactions, that is to cut down the relationship at one end of the distribution.

If we examine the mean scores for the boys and girls we find from ages 9–15 yr. a decrease in times of unprepared reactions to sound and to light, roughly twice those of the warning reactions, in the case of the boys, but of equal amount for girls. This is seen in Table IX. The steady decrease with age of SU and LU as compared with SW and LW for the boys, whereas for girls the differences in these values remain more or less unchanged with advancing age, would tend to make the correlations SU-SD and LU-LD relatively high for boys and not for girls. This is what we find in the correlation table. If the correlations for girls are arranged from highest to lowest and likewise those of the boys are arranged from highest to lowest, most of the variables remain in the same order, or suffer a displacement of one rank only. The correlations SU-SD and LU-LD change from ranks 6 and 7 for the girls to ranks 2 and 3 for the boys and gain in value 0.183 and 0.190 respectively.

There are three negative values of correlations for the girls—SW-SD, SW-LD, and LW-LD. A probable explanation of the negative sign is that in general the girls who react fastest to SW and LW have an SU and LU about average—hence their SD and LD would be greater than average. The boys on the other hand who react fastest to SW and LW have also fast SU and LU reactions and hence also low SD and LD.

Reaction-time and chronological age. As was to be expected there is quite a definite correlation with age for most of the abilities. Moreover the correlation ratios obtained for girls differ in some cases from that of the boys, and in general they are considerably lower; in two instances, SD and LD, the sign is changed. The correlations of CA of both boys and girls for SW are very closely the same (—0.451 and —0.448), while MA with CA is 0.605 and 0.523 respectively. The other correlations are considerably lower for girls than the corresponding values for boys. On the basis of what was said in the previous section this is quite understandable. What is not so evident is the equality between the two SW correlations when prac-

tically every other one is lower for the girls than for the boys. One possible explanation would be that SW reactions having been taken first, the element of novelty predominates, and the difficulty of the task is the same for both sexes, only in the succeeding reactions does any outstanding sex difference show itself. It is true than even in SW there is a sex difference —the boys' reactions are consistently faster than the girls.

These considerations indicated that it might be of advantage to partial

TABLE X

CORRELATIONS WITH CHRONOLOGICAL AGE CONSTANT

		SW	SU	SD	LW	LU	LD
	SU	.577					
	SD	−.225	.658				
Girls	LW	.645	.602	.149			
(N=165)	LU	.494	.788	.501	.640		
	LD	.000	.501	.518	−.133	.665	
	MA	.156	−.118	−.074	−.045	−.105	−.123
	SU	.651					
	SD	−.157	.678				
Boys	LW	.655	.516	−.069			
(N=146)	LU	.500	.735	.453	.510		
	LD	.043	.465	.539	−.277	.741	
	MA	−.116	.008	.095	−.114	.029	.036
	SU	.604					
	SD	−.186	.680				
All	LW	.634	.545	.070			
(N=311)	LU	.467	.771	.520	.572		
	LD	.021	.519	.565	−.180	.724	
	MA	.058	−.042	.010	−.090	−.027	−.028

out the effect of age in the various correlations listed. These values are to be found in Table X, the values of which follow very closely as far as rank is concerned those of Table VIII, in which the first order correlations are given. The new values are, however, all smaller for boys, but are more or less the same for girls. Thus the average correlation for the 11 significant positive values changes for boys, when age is partialled out, from 0.691 to 0.586; for girls the corresponding change is from 0.596 to 0.599. Moreover in Table X there is little or no difference between the correlations of the girls and the corresponding ones of the boys.

When the correlation values, with age constant, of the girls are in turn correlated with those of the boys the r obtained is $0.934 \pm .017$, and the regression line is $Y = 0.977\ x + .006$. When this regression equation is compared with that previously obtained from the first order correlations ($Y = 1.163\ x - 2.06$), it is quite evident that the linear relationship which holds between the raw correlation values of the girls and those of the boys becomes very close to equality when age is held constant.

A study of Table X shows that the correlations of all the *Ss* may be divided into several groups.

(a) Correlations that are valid. Positive and statistically significant correlations were obtained which range from 15–50 times their probable error. They are:

SU-LU	.771	SW-LU	.467
SW-LW	.634	SW-SU	.604
SU-LW	.545	LW-LU	.572

It is to be noted that the correlation for SU-LU is the highest of all and the next highest is SW-LW.

(b) Correlations that are spurious. As the RT differences are obtained by subtracting the warning reactions from the unprepared ones from the same kind of stimuli a spuriously high correlation is obtained.[8] These correlations are SU-SD 0.680 and LU-LD 0.724 in which the spurious element is undoubtedly high. Correlations SU-LD 0.519 and SD-LD 0.565 also contain a spurious element, though in these cases it is an indirect one, as the LD values are partly derived from LU which correlates highly with SU; in the same manner SD-LU 0.520 is partly spurious.

(c) Correlations that are not significant. This group includes all the correlations with MA none of which are much greater than twice their probable error. Also are included the correlations SW-LD 0.021 and SD-LW 0.070.

(d) Correlations that are negative and significant. SW-SD —0.186 and LW-LD —0.180 are about 5 times their probable error. Their spurious element is relatively low and would tend in the opposite direction. The negative sign implies that the higher the RT to warning the lower the RT difference, which is evidently true. A comparison with the values SU-SD 0.680 and LU-LD 0.724 shows that the RT differences are more dependent upon the SU and LU reactions than they are upon the SW and LW reactions, a fact which has already been gleaned from other considerations.

Reaction-time and mental age. While the reaction-time experiment was in progress the *Ss* were all given the Otis Self Administering Test of Intelligence. Form A Intermediate was first given. Those *Ss* who were absent and those who in the opinion of their teachers obtained scores on the first test that for some reason or other were widely different from what these children should have obtained were given form B of the Intermediate examination; the score they made on it was taken as their final score.

In the cases where the Intelligence Test was given some months after reaction-time experiments the scores on the Intelligence Tests were worked out from the tables given by the publishers to an equivalent value for the age at the time of taking the other tests.

It was felt that the Mental Age scores for some of the children in Grade IV were not fair measures of their intelligence, due in some cases to a language handicap, and in others, to the deficiency of the test itself at that low grade. The number of children affected was so small (about 10) a truer result could probably not have been obtained by giving them some other intelligence test and attempting to equate

[8] Cf. H. E. Garrett, *Statistics in Psychology and Education*, 1926, 261.

TABLE XI

FREQUENCY DISTRIBUTIONS OF REACTIONS TO LIGHT AND TO SOUND: CONSTANTS OF THE FITTING OF THESE CURVES TO NORMAL (GAUSSIAN) CURVES

The frequency distributions are based upon the mean reaction-times, measured in sigma, of 311 children. Every *S* gave 30 reactions to sound preceded by a warning, SW; to sound when no warning was given, SU; and to the difference between these two measures for sound, SD. The three values for light, LW, LU, and LD, were obtained in the same manner.

	SW	SU	SD	LW	LU	LD
Mean	165.7	195.9	30.4	180.3	226.0	45.8
S.D.$_{dis.}$	25.6	35.6	25.8	26.4	36.6	27.5
S.D.$_{M.}$	1.45	2.02	1.47	1.50	3.66	1.56
β_1 (Skewness)	.154	.082	.433	.138	.078	.133
β_2 (Kurtosis)	3.11	2.77	2.64	3.02	3.64	2.97
†Pearson's Goodness of Fit χ^2	13.92	4.99	33.28	2.71	5.65	9.22
No. intervals	7	8	11	7	9	11
Pearson's P	.031	.702	.001	.842	.686	.512

† Using a Pearson Type III curve, the closeness of fit was considerably improved. Thus for LD, the new values obtained are: (for Skewness = 0.4) $\chi^2 = 2.80$; and P = 0.94.

their scores with those obtained in the Otis test. Accordingly their scores were grouped as found with those of the other children. The actual scores made on the test, corrected in some cases as just stated, were used for the various correlations; they are equivalent to a MA score.

When the correlations between Mental Age and the various reaction-time tests were tabulated for the girls and boys separately and for all *S*s together the correlations were all negative, and ranged for the girls from zero in the case of SD and LD to —0.251 for SU, which is almost 5 times its probable error. For the boys the correlations, all negative, are of greater value, and all are significant. The largest correlations are LW and SU, as in the case of the girls; they are —0.364 and —0.415.

When chronological age is held constant in the correlations between MA and RT a very different result is obtained. All the correlations are within 3 times their probable error except SW-MA for girls, which is 0.156, a value slightly less than 5 times its probable error. It is to be noted that it is positive, while the tendency of all the other larger correlations for girls and for boys is to be negative. It is doubtful if any significance can be attached to this one value. In general the trend of the SW values, particularly for girls, seems to indicate the presence of another factor, possibly novelty or inadaptation to the experiment, which does not hold in other cases.

One may state then quite definitely that there is no significant correlation between MA and RT when CA is held constant.[6]

Reaction-time and attention. Reaction-time has often been used as a measure of attention. For this purpose there have sometimes been used the rapid-

[6] Easley, *op. cit.*, gives the following correlations: LW-MA — 0.01; LD-MA — 0.00; LW-LD — 0.03.

ity of the simple reaction, sometimes the mean variation of a series of reactions, and sometimes a special method devised by Woodrow and based upon the reciprocal of reaction-time difference. A comparison of these three methods has been discussed elsewhere by the writer and by Easley, who fails to find a common factor of attention in the various tests proposed.[7] It might be apropos to discuss this question in the light of the findings of this investigation.

A study of Table X, giving the correlations of the various factors with CA constant yields several facts which can hardly be interpreted if the reciprocal of RT difference is taken as being a measure of the attention involved. The correlations between SD and LD are, for girls 0.518, for boys 0.539, and for the total group 0.565. These values are considerably lower than the correlations SU-SD and LU-LD for the same groups, they being close to 0.7. If the SD and LD both measure attention it is difficult to see how they would fail to correlate at least as well with themselves as with measures from which they are derived.

The correlation of RT difference either for sound or for light with MA is about zero. Yet evidently attention is involved in intelligence, in fact some are inclined to identify it with intelligence.[8] Must one be satisfied with the explanation that the intelligence tests used involve a different kind of intelligence and hence no correlation between RT difference and MA would be found?

The correlations between SD-SW and LD-LW are low but negative, which seems to imply that the faster an individual reacts the less his power of attention—an absurd conclusion, of course.

The correlations between CA and RT difference are so low as to be insignificant for girls, but are 9 times their probable error for boys. We hardly expect a sex difference as great as this for attention when none has been found for intelligence, memory, etc.

Finally a study of the RT difference frequency distribution curves, discloses several negative SD and LD (8% of the SD and 3% of the LD). If the reciprocal of the RT differences is a measure of attention, these negative values have no meaning. They may however be explained as due to a lack of adaptation to the experiment, and the greater frequency of their occurrence for sound which was taken first confirms this. There is another possible explanation. Individuals need a warning period for their fastest reactions; a preparatory signal given for a shorter or a longer period than the optimum interval yields a slower reaction. Just as one finds quite an individual variability in the length of the reaction, so one

[7] *Op. cit.*

[8] Cf. C. E. Spearman, *Abilities of Man*, 1927, 88.

finds a large variability in the length of the optimum interval. There is then the possibility that for those individuals with a negative RT difference the interval chosen (1–2 sec.) was quite unsuitable; their optimum interval probably being much longer.

One is hence inclined to conclude that the reciprocal of the RT difference is not a satisfactory measure of attention.

Summary

(1) Tentative reaction-time norms to sound and to light have been secured from 165 girls and 146 boys from ages 9 to 16 yr. These norms are based upon 120 reactions from every *S*.

(2) Constants of the curves of the frequency distributions, means and standard deviations are given.

(3) The values of SW, LW, SU, and LU show a steady drop with increasing age, and maintain this order, SW being the fastest reaction.

(4) Fatigue is slightly more apparent than practice in a series of 30 reactions.

(5) The psychological factors involved in reactions to sound and to light are closely similar, but they vary with the mode of giving the stimulus and with sex.

(6) There is a statistically reliable difference between girls and boys for all types of RT except LW. The boys react from 3–5% faster than do the girls.

(7) This difference is probably due to transfer from the greater play activity of boys.

(8) Adolescent girls show a definite retardation of reaction-time which is not noticeable in adolescent boys.

(9) Tables of first order correlations, and correlations with chronological age constant are given for girls, for boys, and for all *S*s.

(10) There is a linear relationship between the correlations of the girls and the corresponding ones of the boys; this relationship approaches equality when CA is held constant.

(11) The correlations from these tables are of several types: those that are (a) valid, positive, and statistically significant; (b) spurious, in whole or part; (c) non-significant; and (d) negative and significant.

(12) There is no correlation between MA and RT.

(13) Woodrow's method of measuring attention by the reciprocal of the RT difference is probably not valid.

THE VOLUME AND INTENSITY OF TONES

By S. S. STEVENS, Harvard University

Experimentation on the problem of tonal volume has tended to yield convincing evidence that pure tones are characterized phenomenally by what has variously been called 'largeness,' 'massiveness,' or 'impressiveness.' Unfortunately, however, the quantitative results obtained by different experimenters have proved less consistent than the introspective results. Each experimenter who has undertaken the task has succeeded in training *O*s to make discriminations of volume with a fair degree of subjective certainty, but the magnitudes of the volumic limens, as determined in various laboratories, have nevertheless varied considerably.

Rich[1] made the first determinations of the volumic limen in 1916. He used the method of constant stimuli to measure the limen for volume in terms of the frequency of the stimulus and found it to be significantly larger than the limen for pitch. In 1919 Rich[2] repeated his experiment and was satisfied that he had confirmed his earlier results, although in this experiment the volumic limens turned out to be smaller in magnitude than those previously obtained. Then, in 1930 Gundlach and Bentley,[3] using the method of constant stimuli, obtained limens for volume which were much smaller than those of either of Rich's determinations and which in some cases approximated the limens for pitch. The variability among the *O*s of this experiment was such as to render definite conclusions hazardous, but, as the authors pointed out, the evidence obtained from this study cast doubt upon the existence of volume as an independent attribute of tones. Recently Zoll[4] has determined volumic limens by Rich's method and found them to be intermediate in size between those reported by Rich and those reported by Gundlach and Bentley.

In addition to the investigation of the relation of volume to pitch, the same method (constant stimuli) has been used to study its relation to phase-differences. Halverson[5] obtained evidence of a peculiar relationship between volume and phase-difference, but the nature of the function was not verified by Gundlach and Bentley.[6] As a matter of fact, Halverson believed that the function which he had obtained

* Accepted for publication May 6, 1933.

[1] G. J. Rich, A preliminary study of tonal volume, *J. Exper. Psychol.*, 1, 1916, 13-22.

[2] Rich, A study of tonal attributes, this JOURNAL, 30, 1919, 121-164.

[3] R. Gundlach and M. Bentley, The dependence of tonal attributes upon phase, this JOURNAL, 42, 1930, 519-543.

[4] P. M. Zoll, The relation of tonal volume, intensity, and pitch, this JOURNAL, 46, 1934, 99-106.

[5] H. M. Halverson, Diotic tonal volumes as a function of differences of phase, this JOURNAL, 33, 1922, 526-534.

[6] Gundlach and Bentley, *op. cit.*

was not due entirely to phase-difference in the two ears, but partly, at least, to intensitive differences in the stimulus.

In 1924 Halverson[7] verified his suggestion that volume is dependent on intensity. His *Os*, trained to recognize volume by using frequency as a variable (Rich's criterion), were able to give consistent judgments of volume when the intensity of the tones was varied by changing the amount of resistance in the secondary of an oscillator circuit. Introspections were obtained which tended to confirm those given by Rich's *Os*. One is forced, however, to question the significance of the quantitative results, because the general trend of the intensitive limens, as Halverson determined them, is in marked disagreement with the results of the later studies of Riesz,[8] Knudsen[9] and others. These experimenters found that the size of the relative limen for intensitive discrimination tends to increase as the sensation level decreases. Halverson's results imply an opposite relationship.

Zoll[10] tried to reproduce Halverson's results using his method, but failed. In fact, Zoll's final conclusion is that, owing to the instability of volume under experimental conditions, its phenomenal existence as an independent attribute of tones should be seriously questioned.

That, very briefly, is the story of tonal volume. We have, on the one hand, adequate introspective evidence that volume is phenomenally a separate and distinct attribute of tonal stimuli; but, on the other hand, experiments in which volume has been used as the basis of judgment have not yielded stable quantitative results. Consequently, there remain grounds for the divergence of opinion which exists among theorists as to the existence, the nature, and the possible physiological basis of the volumic attribute.

This state of affairs is due—so the present experiment indicates—to two principal causes: the failure of experimenters adequately to control the physical stimulus, and the use of the method of constant stimuli.

Sabine[11] cautioned those who use sound stimuli that they must "take account of three factors: the effect of reflection by the walls on the increase in the total intensity of sound in the room; the effect of interference in greatly altering the distribution of this intensity; and the effect of the reaction of the sound vibrations in a room upon the source itself." In other words, the neglect of factors affecting the physical behavior of sound waves is likely to vitiate the results of an auditory experiment. Because of the absence of evidence to the contrary, one is forced to assume that in the experiments on tonal volume no serious account was taken of these disturbing physical factors.

With regard to the second reason for psychologists' failure to obtain convincing quantitative results, it now appears that the application of the method of constant stimuli to the problem was not a wise procedure. There are three reasons why volume is difficult to judge. (1) It is a relatively unfamiliar aspect of tonal stimuli: people

[7] Halverson, Tonal volume as a function of intensity, this JOURNAL, 35, 1924, 360-367.

[8] R. R. Riesz, Differential intensity sensitivity of the ear for pure tones, *Phys. Rev.*, 2nd ser., 31, 1928, 867-875.

[9] V. O. Knudsen, Sensibility of the ear to small differences of intensity and frequency, *ibid.*, 2nd ser., 21, 1923, 84-103.

[10] Zoll, *op. cit.*

[11] W. C. Sabine, *Collected Papers on Acoustics*, 1922, 277 ff.

are not taught from early youth to distinguish it, as they are taught to distinguish pitch and loudness. Consequently, the training of the *O* becomes important. (2) The *O*'s criteria for volume, when once formed, are very instable. They are apt to change in the middle of a series of observations. (3) When two tones are sufficiently unlike in frequency for them to be judged different in volume, the difference in pitch is necessarily larger and more conspicuous than the volumic difference and tends to obscure the latter. These three difficulties render the method of constant stimuli especially unsuitable, because this method provides only for the presentation of tones which differ but slightly in volume and which are heard by the *O* for but a short interval of time. The *O* has no opportunity to manipulate the tones himself, to assure himself as to his criteria by examining large differences in volume, to interrupt a series in order to reaffirm his criteria in case they become uncertain. All this confusion and uncertainty may, as Boring has pointed out,[12] lead to a fourth difficulty: the *O*, without realizing what he is doing, may abandon the judgment of the comparison stimulus with reference to the standard, and may give 'absolute judgments' in which the comparison stimulus is referred, not to the standard, but to the general level of the set of comparison stimuli in use.

In order to overcome the difficulties inherent in the judgment of volume, the present study had recourse to the method of average error, under which the *O* makes what may be called a 'transdimensional equation.' The advantage of this method is that it puts volume, so to speak, into the hands of the *O* and gives him a chance to vary and examine it. He can manipulate the tone at will. He can make certain of his criteria by observing large differences in volume; and he can interrupt a series to reassure himself of his criteria whenever they begin to fail. Moreover, this method lessens the chances that volume will be confused with pitch or loudness, and eliminates the tendency for the judgments to become 'absolute.' It is unfortunate that the attempt of Gundlach[13] to use this method should have failed for the reason that, as I shall try to show, his stimuli were wrongly chosen.

Apparatus

The first consideration in preparing the experimental situation was given to the problem of eliminating to the greatest possible extent the disturbing factors of reverberation and standing waves in the observation room. P. M. Zoll, who had been using the room for his experiments on tonal volume, was convinced that much of the irregularity in his results and in those of other experimenters, should be laid to these factors. Consequently, it was decided to cover the walls and ceiling with a 2-in. thickness of rock wool, and the floor with celotex, 1 in. thick. The coefficients of absorption for these two materials at a frequency of 1024 cycles are 0.82 and 0.72 respectively. The intensity of the standing waves in the room was further diminished by hanging sheets of muslin a few inches apart throughout the room. About 100 sq. yd. of muslin were used in this way. The room measures 7 x 17 x 10 ft.

The generating apparatus was situated outside the observation room. It consisted of a General Radio 508-A oscillator to which was connected the necessary

[12] E. G. Boring, *The Physical Dimensions of Consciousness*, 1933, 117.
[13] R. Gundlach, Tonal attributes and frequency theories of hearing, *J. Exper. Psychol.*, 12, 1929, 187-196.

external capacitance to give any frequency desired in the middle auditory range. By means of a switch, the *E* could readily change the amount of the external capacitance so that two tones of different pitch could be presented alternately to the *O*. This switch[14] was so constructed that it served to eliminate the click from the loud speaker. The two tones were presented to the *O* at the rate of about 40 per min. One of the tones was of a fixed intensity, and will be referred to as the standard tone. The other tone could be varied in intensity by means of a rheostat connected across the output of the oscillator and held in the hands of the *O*. It was possible, then, for the *O* to equate the two alternating tones either in respect of loudness or of volume simply by changing the intensity of one of them. The voltage of each tone was read directly from a General Radio output-meter.

Three different sources of sound were used in the course of the experimentation. Since the results from all three of them are in close agreement, the results are not an artifact of the source used.

One source was a Western Electric earphone, type 509 W. It was mounted about 1 m. behind the *O*'s head in a horizontal position. A small mirror, 5 mm. x 2.5 mm., was suspended in a split silk thread fastened to the prongs of a heavy brass fork which could be lowered over the face of the diaphragm of the earphone. The torsional force of the thread when twisted held one edge of the mirror firmly against the diaphragm at an angle of about 30°. Rotation of the mirror due to the vibration of the diaphragm served, then, to deflect a beam of light reflected by the mirror; and the amount of this deflection served as a measure of the amplitude of vibration of the diaphragm. The purpose of this arrangement was to provide a basis for showing the relationship of volume to the amplitude of the sound-wave.

The second source of sound was a Western Electric loud speaking telephone, No. 560 type, located about 1 m. behind the *O;* and the third source consisted of two Jensen "concert junior" dynamic speaker, 6 in. in diam., mounted so as to fit immediately over the ears of the *O*. This arrangement was utilized for the more extensive series of observation, because it reduced the effect of interference to a negligible amount, and at the same time was free from the troublesome characteristics of earphones, such as the dependence of intensity on the manner in which the phone is held to the ear. It also permits the operation of the speakers at a very low level of intensity with a consequent improvement in the purity of the tones. Furthermore, the sensation level of the tones could be conveniently altered by changing only the potential impressed on the field coils of the speakers.

A signal system between the *O* and the *E* completed the set-up.

Procedure and Results

The *O*s in the experiment were Professor E. G. Boring (*B*), Dr. J. Volkmann (*V*), Dr. C. Goldthwait (*G*), Mr. E. B. Newman (*N*), and Miss S. V. Fladeland (*F*). *B* and *V* had observed in Zoll's experiment, and were consequently trained in the recognition of volume. *G* and *N* were familiar with the concept of volume, but had never tried to observe it under

[14] For a description of this switch see S. S. Stevens, A switch for the presentation of electrically generated tonal stimuli, this Journal, 46, 1934, 138-139.

experimental conditions. *F* was a congenitally blind instructor from the Perkins Institute, who had never heard of volume as a tonal attribute.

In the preliminary experiment, *B, V,* and *N* were each given, in continuous alternation, eight pairs of tones of different frequency with the instructions, in one series, to match them for volume, and in another series for loudness, by changing in each case, the intensity of one of the tones. The tones in each pair were separated in frequency by approximately 50 cycles. The results were,[15] without exception, that the *O* found the two tones equal in volume after the tone of higher frequency had been made more intense than the lower tone. In other words, it was clear that a loud high tone can be experienced as equal in volume to a soft low tone. Although *G* had reported that at first the tones were separated too far in frequency for him to make them equal in volume, after some training with pairs of a lesser difference, he found the match easy to make.

Following this series, *V, G,* and *N* were given a series of eight different tones, each paired with a standard tone of 512 cycles at an intensity of 5.0 volts. The *O*s matched the two tones of each pair in both volume and intensity. The Western Electric earphone was used in this series, so that the relative amplitude of the tones could be determined. The *O*s again matched the tones in volume by increasing the intensity of the higher tone, or by decreasing the intensity of the lower tone, as compared with the value at which the variable tone was judged equal to the standard in intensity. It is this difference, then, in which we are primarily interested, for it shows the amount of intensitive difference which will just compensate for a given difference in pitch when the task is to match two tones for volume. The following are the averages for the three *O*s, when each *O* made 10 intensitive and 10 volumic equations:

Frequency (cycles)	470	480	490	500	512	525	540	565	603
Difference (volts)	—2.00	—1.52	—1.36	—.98	0.00	1.06	2.20	2.86	3.15
Av. amplitude (mm.)	9.0	12.6	15.3	16.5	19.9	22.0	26.3	27.7	29.7

From these data it is clear that the amount of intensitive difference which will compensate for a given difference in pitch varies as the size of the latter difference. From the measures of amplitude it is also evident that the amplitudes of tones of equal volume are not equal, as Boring suggested might be the case.[16] Volume is not at all a simple function of amplitude.

With the dynamic speakers as the source of sound, six tones were matched

[15] The complete data are contained in the author's doctoral dissertation on file in the library of Harvard University under the title of this article.

[16] E. G. Boring, Auditory theory with special reference to intensity, volume, and localization, this JOURNAL, 37, 1926, 164.

TABLE I

RESULTS OF MATCHING 6 TONES TO THE STANDARD FREQUENCY 900 CYCLES AT DIFFERENT SENSATION LEVELS BY G AND N

The values in db. are computed from the thresholds 0.0020, 0.00063, and 0.00016 volts for the 55, 65, and 75 db. levels respectively. Each entry is the average of 10 observations.

	Frequency	G						N					
		55 db.		65 db.		75 db.		55 db.		65 db.		75 db.	
		volts	db.	volts	db.	volts	db.	volts	db.	volts	db.	volts	db.
Intensity	800	1.22	55.71	1.24	65.88	1.16	77.21	1.21	57.15	1.38	66.81	1.44	77.57
	850	1.25	55.92	1.21	65.67	1.08	76.59	1.13	56.06	1.20	65.60	1.27	76.98
	875	1.27	56.05	1.21	65.67	1.08	76.59	1.02	54.73	1.10	64.84	1.09	76.09
	925	1.26	55.99	1.25	65.95	1.13	76.98	1.02	54.15	1.10	64.84	1.02	76.17
	950	1.31	56.32	1.27	66.09	1.13	76.98	1.10	55.49	1.20	65.60	1.19	76.25
	1000	1.25	55.92	1.33	66.49	1.18	77.36	1.20	56.19	1.33	66.49	1.29	77.50
Volume	800	.42	46.44	.59	59.43	.79	73.97	.97	54.24	1.04	64.35	1.03	75.65
	850	.75	51.48	.77	61.74	.81	74.09	.98	54.24	1.00	64.01	1.03	75.74
	875	.96	53.62	.95	63.77	.92	75.19	.93	53.53	.99	63.93	.95	75.29
	925	2.08	60.14	1.65	68.36	1.28	78.06	1.14	55.34	1.23	65.81	1.17	77.06
	950	2.80	62.92	1.94	69.77	1.39	78.77	1.30	57.79	1.47	67.36	1.55	78.20
	1000	4.00	66.02	2.78	72.89	1.61	80.05	1.50	59.51	1.82	69.21	1.89	79.44
Int. minus Vol. (in db.)	800		9.26		6.45		3.24		2.91		2.46		1.92
	850		4.44		3.93		2.50		1.82		1.59		1.24
	875		2.43		1.90		1.39		1.20		.91		.80
	925		−4.15		−2.41		−1.08		−1.19		− .97		− .89
	950		−6.60		−3.68		−1.79		−2.30		−1.76		−1.45
	1000		−10.10		−6.40		−2.70		−3.32		−2.72		−1.94

in intensity and volume to a standard frequency of 900 cycles. The matches were made by *G* and *N* at three different sensation levels. The numerical results are recorded in Table I.

In Fig. 1 the results obtained from *G* are plotted as equal-volume con-

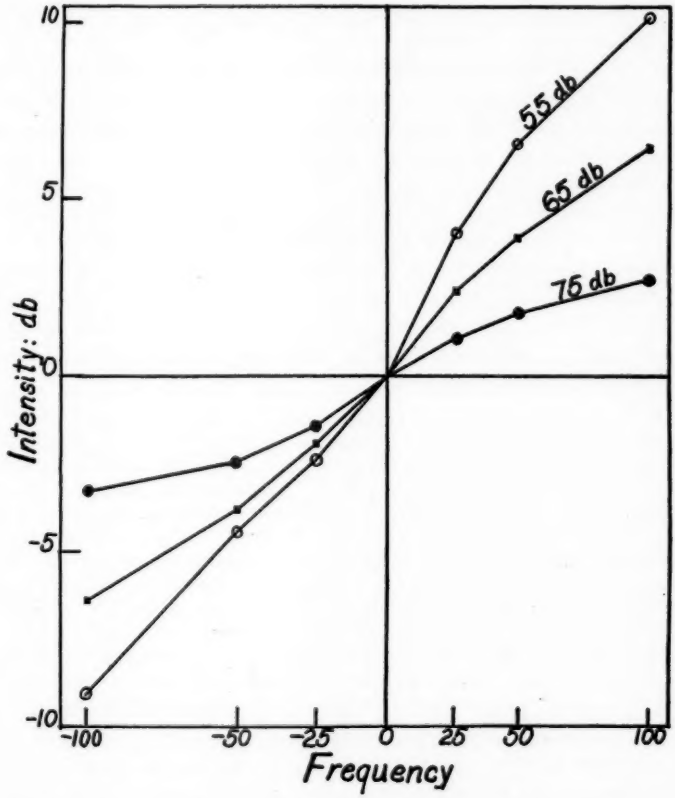

FIG. 1. SHOWING THE MANNER IN WHICH THE AMOUNT OF INTENSITIVE DIFFERENCE NEEDED TO COMPENSATE FOR A DIFFERENCE IN FREQUENCY DEPENDS UPON THE MAGNITUDE OF THE DIFFERENCE IN FREQUENCY AND UPON THE SENSATION LEVEL

The values from which these curves are drawn were obtained by subtracting the values (in db.), at which the *O* reported the respective tones equal to the standard tone (900 cycles) in volume, from the values at which he reported them equal in intensity.

tours with sensation levels as parameters. *N*'s results yield similar functions except that the absolute values are different, as can be seen from an inspection of the data in Table I. The manner in which the slope of the curves decreases as the sensation level increases suggests that at very high sensation levels the volume of all tones would be equal. This possibility has not been thoroughly investigated as yet. Listening to such loud tones is a very unpleasant occupation.

Fig. 1 shows that at low sensation levels the amount of intensitive difference necessary to compensate for a given difference in frequency is greater than at high sensation levels. This function means that the difference in volume between two given tones is greater at lower sensation levels. From this fact it follows that the higher tones increase in volume more rapidly than low tones when sensation level increases. Both low and high tones increase in volume when their intensity is increased, but the high tones increase in volume more rapidly. For instance, at 40 db. a tone of 800 cycles is phenomenally larger than a tone of 1000 cycles, but at 100 db. they have the same, or nearly the same, volume. Hence, the 1000-cycle tone increases more in volume than the 800-cycle tone as the sensation level is increased from 40 to 100 db.

It is interesting to note that the curves of Fig. 1 indicate the probable form that equal-volume contours would have in relation to the familiar diagram of the auditory area as drawn by Fletcher.[17] Equal loudness contours have been determined by Kingsbury[18] and are reproduced on Fletcher's diagram, along with equal sensation level contours. From *G*'s data it appears that equal-volume contours would be formed as follows. At the sensation level of 100 db. the equal-volume contour would approximately coincide with the contour representing equal sensation level. Then, as the sensation level decreases, the slope of the equal-volume contours would increase relative to the slope of the equal-loudness contours, until at points near the threshold of hearing the slope would reach a maximum. The precise mapping of these contours would involve the exploration of the entire "auditory area" by the method used in this experiment. The above observations serve only to indicate that such a mapping is possible and the probable form which it would take.

The coefficients of variation of the results shown in Table I are presented in Table II. In every case the average variability is greater among the judgments of volume than among those of intensity for the same sensation level. In the case of *G* there is a pronounced tendency for the lower sensation levels to yield larger average deviations, both for volume and intensity, but the large variability in the results of *N* at the level of 65

[17] H. Fletcher, *Speech and Hearing*, 1929, 162.
[18] B. A. Kingsbury, A direct comparison of the loudness of pure tones, *Phys. Rev.*, 2nd ser., 29, 1927, 588-600.

db. makes this tendency less clear-cut for him. If we take these coefficients to be indicative of the relative sizes of the relative differential limens, it becomes apparent that the results are in agreement with those of Riesz,[19] who showed that the relative differential limen for intensity increases as the sensation level is lowered. Shower and Biddulph[20] found an analogous situation to hold for pitch. Then, since judgments of both pitch and intensity are made with a larger probable error at the lower sensation levels,

TABLE II

COEFFICIENTS OF VARIATION OF THE RESULTS OBTAINED FROM G AND $\mathcal{N}$ AT 3 DIFFERENT SENSATION LEVELS

Each entry represents the variation in 10 equations made at each frequency and sensation level.

	Frequency	Intensity			Volume		
		Sensation level					
		55 db.	65 db.	75 db.	55 db.	65 db.	75 db.
G	800	9.59	5.97	6.03	29.52	22.88	8.22
	850	7.91	5.12	5.09	19.87	12.86	6.54
	875	7.87	4.13	4.44	14.48	7.16	6.85
	925	7.62	3.92	2.65	12.39	9.76	7.11
	950	7.02	5.51	2.30	7.75	10.88	5.76
	1000	9.20	6.24	7.37	7.25	8.06	7.45
	Av.	8.20	5.15	4.65	15.30	11.93	6.99
$\mathcal{N}$	800	4.31	8.62	5.29	4.56	10.38	5.88
	850	2.99	2.42	3.01	7.67	1.59	7.04
	875	7.61	8.32	2.25	4.32	7.17	6.77
	925	4.11	7.18	3.30	3.08	9.02	3.77
	950	6.72	7.33	5.55	8.84	7.00	4.62
	1000	1.29	10.53	4.92	10.42	11.32	4.20
	Av.	4.51	7.33	4.05	6.48	7.74	5.38

and since the judgment of volume is a function of both the frequency and the intensity of the stimulus, it seems reasonable to expect the variability of the volumic judgments to vary as it does in the case of G. In short, the relative differential limens of pitch, loudness, and volume all increase in size as the sensation level decreases.

It will be noted that the contour curves of Fig. 1 tend to be sigmoid in form. This tendency is present in every one of twelve different curves determined during the course of this experiment. The reason is not known. Perhaps the *O*s tend to over-compensate for the volumic difference caused by a difference in pitch when that difference is small.

With regard to the question as to whether volume is more dependent upon the frequency of the stimulus or upon its intensity, an interesting

[19] Riesz, *op. cit.*

[20] E. G. Shower and R. Biddulph, Differential pitch sensitivity of the ear. *J. Acoust. Soc.*, 3, 1931-32, 275-287.

calculation can be made from the data of Table I. Let us take the case of *N*. At a sensation level of 65 db. this *O* used about 2.5 db. of intensity to compensate for a difference of 100 cycles. Now, from Riesz's results[21] it is seen that 2.5 db. at this sensation level is equivalent to about four j.n.d. of intensity, and from the data of Shower and Biddulph[22] it appears that 100 cycles at the frequency-level of 1000 cycles represents about 30 j.n.d. of pitch. It seems, therefore, that one j.n.d. of intensity is equivalent to almost 8 j.n.d. of pitch in altering the volume of a tone for this *O*. The ratio for *G* would be only about half as large. In general, however, one can conclude that a j.n. change in the intensity of a tone has a greater effect on its volume than a j.n. change in pitch.

It now seems probable that negative results were obtained by Gundlach[23] because the tones were too widely separated in frequency. In the present experiments points were found (usually about 300 cycles removed from the standard stimulus) at which the *O*s had difficulty in matching the tone to the standard tone in volume. It was unfortunate that Gundlach gave his *O*s frequencies of such wide separation as 100-400, 400-800, and 800-2000 cycles, for when the *O*s tried to equate the two tones in volume the intensity of the higher tone had to be made so great that it was accompanied by "violent starts and mutterings on the part of all the observers." Even under these conditions a few equations were "forced out of the subjects," as Gundlach phrased it; but the separation was obviously too great.

The case of *F*, the congenitally blind *O*, is of special interest because it shows that, contrary to the implication of Banister,[24] blind persons can be trained to judge volume. It also shows that the judgment is not necessarily made in terms of visual imagery. The same procedure was used as was employed with the other *O*s, and the results, although limited to fewer observations, were substantially similar to those already described. A function similar to those of Fig. 1 was obtained. At the beginning of the experiment *F* reported that concrete kinaesthetic imagery was being used, but at the end of a series of 32 judgments she stated that it had become possible for her to abstract the 'size' of the tones from their pitch and loudness without the aid of concrete imagery.

The foregoing experimental results show that what has been called *volume* is a phenomenal dimension of pure tones and that this dimension is a complex function of both the frequency and the amplitude of the

[21] Riesz, *op. cit.*
[22] Shower and Biddulph, *op. cit.*
[23] R. Gundlach, Tonal attributes and frequency theories of hearing, *J. Exper. Psychol.*, 12, 1929, 187-196.
[24] H. Banister, Auditory theory: A criticism of Professor Boring's hypothesis, this JOURNAL, 38, 1927, 436-440.

stimulus. Since, in fact, Kingsbury[25] showed that loudness is a complex function of the two dimensions of the stimulus, and in view of the suggestions of Troland[26] and Ogden[27] that pitch itself depends upon *both* frequency and amplitude, it may be stated that no psychological dimension need be a simple correlate of a single dimension of the stimulus.

The relation of volume to the localizability of tones seems, on the basis of introspective evidence, to be fairly intimate. The low tones are reported as ill-defined and indefinite as to position in space, whereas the high tones are usually more definite as to form and localization. Boring[28] suggested that volume may determine localization, but Banister[29] reversed the argument and said that the notion that tones have volume "may be only tantamount to a confession of failure of discrimination [of localization]." An adequate study of the relation of localizability to pitch and intensity would probably clear up this issue.

Finally, we might ask what tonal volume is. We are justified in saying that volume is a phenomenal dimension of tones. It is a dimension in terms of which an *O* is able consistently to make discriminable judgments, and in terms of which he is able to equate two tones which differ in respect of every other phenomenal dimension. It is better to call it a *dimension* than an *attribute,* because the word *attribute* has historically more specific connotations. The word *dimension* signifies that a tone can have any number of 'sizes;' that is to say, volume is one of the aspects in terms of which a tone can undergo continuous variation. *Volumic dimension* means a continuum, the entire range of specific values of volume that tones might have. The term *variable* can be applied to the specific volumic values, V, that a tone may have. Consequently, we can say that V varies in the volumic dimension and is a function of the frequency and the intensity of the stimulus.

Conclusions

(*1*) The previous investigations of the facts of tonal volume suffer from the fact that stimulus-conditions were not adequately controlled, and from the use of the method of constant stimuli which seems not to have been adequate to the problem. As a result, the quantitative data of the earlier experiments show little agreement and reliability. All of these experiments,

[25] Kingsbury, *op. cit.*
[26] L. T. Troland, *Psychophysiology,* II, 1930, 222.
[27] R. M. Ogden, *Hearing,* 1924, 258.
[28] Boring, *op. cit.,* this Journal, 37, 1926, 168.
[29] Banister, *op. cit.*

however, have left us with introspective evidence that volume is a discriminable aspect of tonal stimuli.

(*2*) The method of average error has shown itself to be suitable to the study of the volumic dimension of tones, largely because it provides an opportunity for the *O* to manipulate the tones, and to stabilize his criteria by voluntary reference to large differences of volume.

(*3*) Under this method the *O* makes an equation of volume by varying the relative intensity of two tones separated by a constant difference of pitch.

(*4*) Tones of different pitch can be equated in volume by trained *O*s. The variability of these equations is greater when the tones are equated in volume, than when they are equated for intensity.

(*5*) The amount of intensitive difference which is required to compensate for a given difference of pitch, in order to make two tones equal in volume, depends upon the sensation level at which the equation is made. More compensatory intensitive difference is required at the lower sensation levels.

(*6*) The congenitally blind can be trained to observe volume.

(*7*) Pitch, loudness, and volume may each be a complex function of the two variables of the stimulus, frequency, and amplitude.

(*8*) The relation of the volume of tones to their localizability and the relation of volume to psychological space remain undetermined.

EMC TIONAL REACTIONS CONNECTED WITH DIFFERENCES IN CEPHALIC INDEX, SHADE OF HAIR, AND COLOR OF EYES IN CAUCASIANS

By GEORGE M. STRATTON, University of California

The present part of a more extended study[1] deals with the question whether the emotions of persons will be found to differ when we pass from those who have certain physical features to those who have other physical features. The study may prove to be the more significant because the bodily features chosen for our attention are among the signs often used to distinguish certain chief strains within the Caucasian race. The features selected are the cephalic index, the shade of the hair, and the color of the eyes.

The investigation in the course of several years involved somewhat over 1000 persons as subjects, both women and men, all of them Caucasian students in the University of California.[2] Measurements of each person were made from which the cephalic index could be computed; and there were observations which allowed each person to be classed as of light, medium, or dark hair, using for each observation a scale of actual samples of hair, to escape some of the errors of a purely subjective judgment.[3] There were observations, finally, to determine whether the eyes were

* Accepted for publication May 15, 1933.

[1] G. M. Stratton, Emotion and the incidence of disease, *J. Abn. & Soc. Psychol.*, 21, 1926, 19-23; Anger and fear: Their probable relation to each other, to intellectual work, and to primogeniture, this JOURNAL, 39, 1927, 125-140; Emotion and the incidence of disease. The influence of the number of diseases, and the age at which they occur, *Psychol. Rev.*, 36, 1929, 242-253.

[2] Of students not Caucasian, it is hoped that a report may be made at some later time.

[3] In the scale here used, the lower limit of 'light' hair is approximately the shade of sample No. 9 of the Fischer Scale; the upper limit of 'dark' hair is approximately the shade of No. 6 of the Fischer Scale. 'Medium' hair includes all hair judged to lie between these limits. These limits were determined by combining the judgments of three or more members of the psychology department in regard to samples of the hair of 40 students chosen at random. The samples were arranged in order from lightest to darkest (the extremes of shade we may designate as *a* and *d* respectively). The observer was then requested to select two other shades (*b* and *c*) in the array of samples, such that their relation to the extremes would be judged by him to accord with the formula $a:b::b:c::c:d$. The aim thus was to have equal steps of apparent difference in shade. There was of course no intention to have the array fall into groups having equal numbers of samples. Indeed when the 40 samples were grouped by the method just described, there were 9 samples of 'light' hair, 13 samples of 'medium,' and 18 samples of 'dark.' In Table I it will be seen that by this scale there were in our student population about equal numbers of persons having 'light' and 'medium' hair, while a far larger group is of those with 'dark' hair.

blue or brown or a mixture of blue and brown. These three classes of eyes will hereafter, for brevity, be called light, dark, and medium. The emotional reactions were recorded by a method whose full description has appeared elsewhere.[4] In essence it was that directly after having found himself in one of certain designated situations, *O* recorded the degree of his reaction according to a definite scale of such reactions, each degree of which was briefly indicated by definite characteristics, in printed instructions given him. From the 1000 and more persons already mentioned, a crude measure was thus obtained of about 9000 particular emotional reactions of fear or anger, immediately after their occurrence in carefully specified situations of real life. From the crude measures made by the *O*s, more refined measures were derived by a computation described in the article just cited.

TABLE I

MEAN EMOTIONAL RESPONSES WITH P.E., OF *O*S DISTINGUISHED BY HAIR SHADE

Shade of hair	Fear			Anger		
	No. *O*s	No. responses	Mean	No. *O*s	No. responses	Mean
Light	302	863	98.21 ± .57	326	1092	98.74 ± .51
Medium	331	1085	97.67 ± .48	347	1468	101.59 ± .41
Dark	484	1789	99.89 ± .39	553	2702	100.84 ± .30

TABLE II

DIFFERENCES, WITH P.E., IN EMOTIONAL RESPONSES OF *O*S DISTINGUISHED BY HAIR SHADE

Shades of hair of groups compared	Fear			Anger		
	Diff.	P.E.$_{diff.}$	Diff./P.E.$_{diff.}$	Diff.	P.E.$_{diff.}$	Diff./P.E.$_{diff.}$
Light, Medium	0.54	.75	0.72	2.85	.65	4.36
Light, Dark	1.68	.69	2.43	2.10	.59	3.55
Medium, Dark	2.22	.62	3.59	0.75	.51	1.47

Coming now to the findings by this procedure, there appear only small emotional differences between the average of any one group and the average of any other. Many of the differences are indeed so small, both in themselves and in relation to their probable errors, that they may most suitably be ascribed to chance. But some of them though small, are statistically trustworthy. In many cases the difference exceeds 3 times its probable error, occasionally running as high as 6 or 8 times. Such differences may be regarded as probably due, not to accident, but to some real although slight contrast in the emotional tendencies of the groups compared.

It may be well to look, first, at what appears in the emotional reactions connected with each of the three physical variables separately; that is, with variations in shade of hair, disregarding eye-color and shape of head; and similarly with these other features, each in its turn.

Thus, taking the shade of hair, there appears to be a difference, with respect to

[4] *Op. cit.*, this JOURNAL, 39, 1927, 125-140.

fear, between the group composed of the medium-haired and the group composed of the dark-haired, a difference which is more than thrice as large as its P.E. (Table II). With respect to anger, there appear differences between the light-haired and the dark-haired, and between the light-haired and the medium-haired, which are from 3 to 4 times their P.E. (Table II). The medium-haired have a slightly less intense fear than have the dark-haired; the medium-haired and the dark-haired have a slightly more intense anger than have the light-haired (Table I).

TABLE III

MEAN EMOTIONAL RESPONSES WITH P.E., OF *Os* DISTINGUISHED BY EYE COLOR

Color of eyes	Fear			Anger		
	No. *Os*	No. responses	Mean	No. *Os*	No. responses	Mean
Light	454	1489	98.91 ± .42	509	2106	101.71 ± .37
Medium	478	1605	99.18 ± .43	510	2312	99.84 ± .31
Dark	185	643	97.97 ± .62	207	844	99.97 ± .58

TABLE IV

DIFFERENCES WITH P.E., IN EMOTIONAL RESPONSES OF *Os* DISTINGUISHED BY EYE COLOR

Color of eye of groups compared	Fear			Anger		
	Diff.	$P.E._{diff.}$	$Diff./P.E._{diff.}$	Diff.	$P.E._{diff.}$	$Diff./P.E._{diff.}$
Light, Medium	.27	.60	.45	1.87	.48	3.87
Light, Dark	.94	.75	1.26	1.74	.69	2.53
Medium, Dark	1.21	.75	1.60	.13	.66	.20

TABLE V

MEAN EMOTIONAL RESPONSES WITH P.E., OF *Os* DISTINGUISHED BY CEPHALIC INDEX

Cephalic index	Fear			Anger		
	No. *Os*	No. responses	Mean	No. *Os*	No. responses	Mean
69–77*	333	1065	101.73 ± .53	382	1683	102.87 ± .38
78–80†	411	1423	96.92 ± .41	446	1914	98.60 ± .36
81–89‡	373	1249	98.62 ± .47	398	1665	100.64 ± .40

* Here and later, this expression means the entire group of *Os* with a cephalic index of 77.49 or less.

† Here and later, this expression means the entire group of *Os* with a cephalic index of 77.50 to 80.49, inclusive.

‡ Here and later, this expression means the entire group of persons with a cephalic index of 80.50 or more.

Next, taking eye-color, the light-eyed are slightly more intense in their anger than are the medium-eyed (Table III), the difference between the average responses of the two classes being above thrice its P.E. (Table IV). No other statistically trustworthy difference appears.

As regards the shape of the head, the narrow-headed are more intense both in their anger and in their fear than are either of the other groups (Table V): in like situations the narrow-headed become angrier or more afraid than do those whose heads are of medium proportions or broad. The medium heads show the

least intense fear and anger, of our three groups; the broad heads fall between the medium and the narrow. The difference, in all cases but one, is statistically reliable (Table VI).

TABLE VI

DIFFERENCES, WITH P.E., IN EMOTIONAL RESPONSES OF Os DISTINGUISHED BY CEPHALIC INDEX

Indices of groups compared	Fear			Anger		
	Diff.	P.E.$_{diff.}$	Diff./P.E.$_{diff.}$	Diff.	P.E.$_{diff.}$	Diff./P.E.$_{diff.}$
69–77, 78–80	4.81	.67	7.18	4.27	.52	8.16
69–77, 81–89	3.11	.71	4.39	2.23	.55	4.04
78–80, 81–89	1.70	.62	2.72	2.04	.54	3.79

If these results should be supported by other studies, and they of course await such confirmation, several features would attract one's attention.

First, the three bodily features, at least as here used, are not equally important as indicators of the average emotional reactions to be expected from a large number of persons. If one reviews the tables given thus far it will be seen that the average reaction in fear or anger when we pass from one head-shape to another head-shape usually varies by a greater amount than when we pass from one hair-shade to another, or from one eye-color to another. As between these latter features, the emotional differences between our groups distinguished by shade of hair are greater than the emotional differences between our groups distinguished by color of eyes. It is of course possible that by some other classification based on hair-shade or by some other classification based on eye-color or shape of head the outcome would be different. But considering the differences here chosen, the bodily features vary in importance as indices of the emotional responses to be expected, and in the following order from great to less: (1) cephalic index; (2) shade of hair; (3) color of eyes.

As between fear and anger, there appears little, if any, constant difference in the power of the different bodily features to indicate what is to be expected. The amount of difference in *fear* connected, say, with contrasts in cephalic index, is, on the whole, not strikingly different from the amount of difference in *anger* connected with such head-contrasts. Similarly for the other two bodily features. The average difference in fear connected with difference of cephalic index is very slightly more than that in anger. The average difference in fear connected with difference in shade of hair or color of eyes is slightly less than that in anger.

Furthermore, it is of interest that the emotional differences are most pronounced, not between groups whose physical features stand in strongest contrast, but between groups physically closer together. Thus with respect to shade of hair, the largest and statistically most trustworthy emotional difference is not between the light-haired and the dark-haired, but between the medium-haired and one or other of the extremes (Table II). Or again, with respect to eyes, the most pronounced emotional difference is not between the light-eyed and the dark-eyed, but between the medium-eyed and one or other of the extremes (Table IV). Or taking our third basis of classification, shape of head, the clearest emotional difference is not between the polar opposites, the narrow-headed and the broad-headed, but between the narrow-headed and those whose heads are of medium cephalic index (Table VI).

TABLE VII

MEAN EMOTIONAL RESPONSES, WITH P.E., OF *Os* DISTINGUISHED BY COMBINATIONS OF CEPHALIC INDEX, HAIR SHADE, AND EYE COLOR

Index	Hair	Eye	Fear			Anger		
			No. *Os*	No. responses	Mean	No. *Os*	No. responses	Mean
69–77	Light	Light	62	197	99.53 ± 1.09	69	250	101.94 ± 1.08
81–89	Medium	Medium	48	158	95.13 ± 1.10	49	224	105.08 ± .91
69–77	Dark	Dark	35	102	101.27 ± 1.43	45	193	104.66 ± .93

TABLE VIII

DIFFERENCES, WITH P.E., IN EMOTIONAL RESPONSE OF *Os* DISTINGUISHED BY COMBINATIONS OF CEPHALIC INDEX, HAIR SHADE, AND EYE COLOR

Combinations in the groups compared						Fear			Anger		
Index	Hair	Eye	Index	Hair	Eye	Diff.	P.E.$_{diff.}$	Diff./P.E.$_{diff.}$	Diff.	P.E.$_{diff.}$	Diff./P.E.$_{diff.}$
69–77	Light	Light	81–89	Medium	Medium	4.40	1.55	2.84	3.14	1.41	2.22
69–77	Light	Light	69–77	Dark	Dark	1.74	1.80	.97	2.72	1.46	1.86
81–89	Medium	Medium	69–77	Dark	Dark	6.14	1.80	3.40	.42	1.30	.32

One may next ask whether the present findings point to a difference in the emotional disposition of the three more frequently recognized strains within the Caucasian race; namely, the Nordic, Alpine, and Mediterranean. There are indications of a difference.

In the Nordic stock, it will be recalled, a relatively small cephalic index (that is, a narrow head) is joined with light hair and light eyes; in the Alpine, a relatively large cephalic index (a broad head) is joined with medium hair and medium eyes; and in the Mediterranean stock, a small cephalic index (again, a narrow head) is joined with dark hair and dark eyes. Among the persons here studied, the number of individuals who combine all three of the features in any one of the ways just described is relatively small, ranging from 35 to 69 persons; and the indication from groups so small is of course unsatisfactory. But such as the numbers are, they suggest a difference with respect to fear between our 'Alpines' and the others; the 'Alpines' having a mean score for fear that is lower than that of our 'Nordics' or our 'Mediterraneans' (Table VII). But only between 'Alpine' and 'Mediterranean' is the difference over thrice its P.E. (Table VIII).

When the grouping is based on two features instead of three, the groups of course become somewhat larger, and there appears a larger number of emotional differences that are statistically satisfactory. Thus taking only head-shape and color of eyes as indicators of the different stocks among the Caucasians,[5] our 'Alpines' have a lower emotion score for both fear and anger than either our 'Nordics' or our 'Mediterraneans' (Table IX); and for the comparisons between 'Alpine' and 'Nordic,' and between 'Alpine' and 'Mediterranean' there is, with respect to both

TABLE IX

MEAN EMOTIONAL RESPONSES, WITH P.E., OF *Os* DISTINGUISHED BY CERTAIN COMBINATIONS OF CEPHALIC INDEX AND EYE COLOR

Combination		Fear			Anger		
Index	Eye	No. *Os*	No. responses	Mean	No. *Os*	No. responses	Mean
69–77	Light	142	453	101.23 ± .75	166	716	105.63 ± .61
81–89	Medium	160	541	97.85 ± .68	170	795	101.58 ± .54
69–77	Dark	47	128	103.88 ± 1.23	54	224	104.99 ± .93

fear and anger, a fairly satisfactory relation between the differences and their P.E. (Table X). The indications from these larger groups based on head-shape and color of eyes thus strengthen and extend the indications given by the smaller groups based on three features.

When, further, we base our 'racial' classification on a different combination of two features; namely, on shape of head and shade of hair,[6] instead of head-shape and

[5] A combination of cephalic index 69-77 and light eyes is here assumed to come closer to a 'Nordic' strain than would any other combination of head-shape and color of eyes here used; of cephalic index 81-89 and medium eyes, to an 'Alpine' strain; and of cephalic index 69-77 and dark eyes, to a 'Mediterranean' strain.

[6] A combination of cephalic index 69-77 and light eyes is here assumed to come closer to a 'Nordic' strain than would any other combination of the head-shapes

color of eyes, the group in every case comprises more than 100 persons. Some of the former emotional differences among our groups now reappear dependably, and additional differences appear. Again our 'Alpines' have lower fear scores than our 'Mediterraneans' and 'Nordics;' and the 'Alpines' have lower anger scores than the 'Mediterraneans,' but not lower than the 'Nordics' (Table X). Now the 'Nordics' and the 'Mediterraneans' are different from each other by relatively large and statistically satisfactory amounts, the 'Nordics' showing less intense fear and less intense anger than our 'Mediterraneans' (Table XI). That our 'Nordics' are less intense than our 'Mediterraneans' in their responses of fear was faintly indicated in Table IX; and that the 'Nordics' are less intense than the 'Mediterraneans' in both fear and anger was faintly indicated in Table VII. These faint indications now become clear.

Briefly, then, our samples of the Caucasian stocks, as here defined, might, if we be guided solely by such differences as seem trustworthy statistically, be ranked as follows with respect to their fear and anger, beginning with the stock showing the most intense reactions: (1) 'Mediterranean;' (2) 'Nordic;' (3) 'Alpine.'

In some respects this order is in accord with the frequent opinion that the South European (for example, the Italian or the Spaniard) is more emotional than the European of the Center or of the Northwest; and with the opinion, perhaps less frequent, that the Alpine is the most phlegmatic of the three European stocks. The present indications lend no support, however, to the opinion that the race of the long-headed, light-haired, blue-eyed folk of Northwestern Europe are the most courageous of all the Caucasians, and the most terrible in their wrath. But if the present indications should prove correct, the true believer in the mission of the Nordic people to rule the world may easily shift his ground, and find the superiority of the Nordic, emotionally, to lie in the very fact of his mid-place, in his possessing the virtues of the mean, being neither phlegmatic like the Alpine nor gusty like your Mediterranean.

But in connection with these indications of an emotional contrast in the three European stocks more commonly recognized, another indication should be noted. It is that in the population here studied, an equal if not greater emotional contrast is found between some groups as just defined and another group whose physical features do not fit the usual description of European stocks. I have in mind a group marked by dark hair and eyes and a medium cephalic index, 78-80. In its dark hair and dark eyes this nameless group seems to be Mediterranean, and in its head-shape also it seems to be within the border of the Mediterranean stock as often defined. Yet this nameless group is emotionally at the opposite pole from the 'Mediterranean' as defined in the present study. Whereas the 'Mediterraneans,' in general, display the intensest emotions of all the three groups named, yet the nameless group (Table XIII) reveals a lower average degree of fear and of anger than does even the lowest of the three; namely, the 'Alpine.' The nameless group therefore stands in extreme contrast, emotionally, to the very group to which it is nearest in its physical marks, the 'Mediterranean.' Emotionally this group marked off from the others by mesocephaly alone, or by mesocephaly combined with either dark eyes

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and shades of hair here used; of cephalic index 81-89 and medium hair to an 'Alpine' strain; and of cephalic index 69-77 and dark hair to a 'Mediterranean' strain.

TABLE X

DIFFERENCES, WITH P.E., IN EMOTIONAL RESPONSES OF *Os* DISTINGUISHED BY CERTAIN COMBINATIONS OF CEPHALIC INDEX AND EYE COLOR

Combinations				Fear			Anger		
Index	Eye	Index	Eye	Diff.	P.E.$_{diff.}$	Diff./P.E.$_{diff.}$	Diff.	P.E.$_{diff.}$	Diff./P.E.$_{diff.}$
69–77	Light	81–89	Medium	3.38	1.01	3.34	4.05	.81	4.97
69–77	Light	69–77	Dark	2.65	1.29	2.05	.64	1.11	.58
81–89	Medium	69–77	Dark	6.03	1.41	4.29	3.41	1.08	3.17

TABLE XI

DIFFERENCES, WITH P.E., IN EMOTIONAL RESPONSES OF *Os* DISTINGUISHED BY CERTAIN COMBINATIONS OF CEPHALIC INDEX AND HAIR SHADE

Combinations				Fear			Anger		
Index	Hair	Index	Hair	Diff.	P.E.$_{diff.}$	Diff./P.E.$_{diff.}$	Diff.	P.E.$_{diff.}$	Diff./P.E.$_{diff.}$
69–77	Light	81–89	Medium	4.09	1.29	3.16	2.03	1.17	1.74
69–77	Light	69–77	Dark	5.04	1.27	3.96	6.34	1.02	6.22
81–89	Medium	69–77	Dark	9.13	1.14	8.02	4.31	.91	4.73

TABLE XII

MEAN EMOTIONAL RESPONSES, WITH P.E., OF *Os* DISTINGUISHED BY CERTAIN COMBINATIONS OF CEPHALIC INDEX AND HAIR SHADE

Combination		Fear			Anger		
Index	Hair	No. *Os*	No. responses	Mean	No. *Os*	No. responses	Mean
69–77	Light	101	288	99.67 ± 1.00	105	359	97.98 ± .87
81–89	Medium	107	371	95.58 ± .82	107	428	100.01 ± .78
69–77	Dark	132	486	104.71 ± .79	161	790	104.32 ± .53

TABLE XIII

MEAN EMOTIONAL RESPONSES, WITH P.E., OF *Os* DISTINGUISHED BY A CEPHALIC INDEX 78–80, ALONE, AND IN COMBINATION WITH DARK HAIR AND DARK EYES

Distinguished by	Fear			Anger		
	No. *Os*	No. responses	Mean	No. *Os*	No. responses	Mean
Index 78–80	411	1423	96.92 ± .41	446	1914	98.60 ± .36
Index 78–80 and Dark Hair	190	712	95.39 ± .56	209	1055	97.69 ± .46
Index 78–80 and Dark Eyes	69	294	93.99 ± .80	76	317	96.65 ± .94
Index 78–80 and Dark Hair and Dark Eyes	50	220	92.09 ± .88	54	239	97.18 ± 1.06

or dark hair or both, seems to call for the special attention of investigators in other fields. Some difference in racial strain not yet clearly recognized may lie at the root of such a psychologically marked group. Or the darkness at present encompassing this group may be dispelled at least in part by further studies in the different temperaments which go with different 'types' of bodily structure.[7] Or race and bodily structure may unite to give us light.

But whether the explanation of the facts indicated be racial or not, the main facts themselves seem fairly clear, that differences in emotion are connected with differences in shape of head, in shade of hair, in color of eye. It seems probable, further, that the main connection between the two orders of difference is a native connection. In the view taken by the present writer, the differences in body-structure here considered are the visible signs of deeper somatic differences, presumably in nervous structure and in nervous functioning. These deeper bodily differences, in their turn, are connected with differences in psychic disposition, of which the emotional reactions here observed are the modes of display.

Some persons will of course reject outright this bare outline of an explanation in so far as it looks toward an innate variation in psycho-somatic disposition. They will prefer to regard the variation not as innate but as acquired. No evidence is

[7] Cf. E. Kretschmer, *Körperbau und Charakter*, 1921.

yet at hand to prove them wrong, since the cause of the observed connection between anger and, say, a particular shape of head, may conceivably lie in some environmental difference acting upon those who in the present study had this particular shape of head. If one prefers to look first of all to individual experience or social training or to climate or food, or to other peculiarities in the individual's environment, psychic or physical, as the principal if not sole source of a man's difference from his fellows, then one can follow that course here. But in the present case I find this a hard course to follow. For it means that among the university students in the present study, the hundreds of persons with, say, a cephalic index of 78-80, although they had lived scattered through communities in many states, yet they had been subject to an environment importantly and systematically the same, while importantly and systematically different from the environment of those whose cephalic index is 81-89 or 69-77. Similarly of the students whose hair or eyes differ from those of their fellows. Students with hair of medium shade may, it is true, have lived in surroundings importantly different from the surroundings of students with lighter or darker hair. But the probability of it seems to the present writer small indeed.

On the contrary the present evidence invites one to recognize that the innate emotional equipment in any large number of persons is connected with certain differences in the features of the head and hair and eyes. The interest in these connections is heightened because the variations in bodily features here used are, in some cases, although not in all, the variations which mark off certain stocks of the Caucasians.

SUMMARY

(*1*) About 9000 particular instances of either fear or anger were systematically observed in prescribed situations by 1000 and more students all of whom were of the Caucasian race.

(*2*) Head-measurements of these persons were made, from which were derived their cephalic indices; and the persons were observed and classified as to their shade of hair and color of eyes.

(*3*) Significant differences both in fear and in anger were found to be connected with differences in each of the three bodily features named above.

(*4*) The most pronounced differences in emotion occur, in general, not between groups whose physical features stand in strongest contrast, but between groups physically closer together, for example, between the medium-haired and the dark-haired, or between those with medium and those with small cephalic index.

(*5*) The three bodily features as used in the present study are not of equal importance as indices of emotional disposition. Their rank in importance is: first, cephalic index; second, shade of hair; and third, color of eyes.

(*6*) Significant differences of emotion were found to be connected with

those combinations of bodily features which have been thought to point, respectively, to Nordic, Alpine, and Mediterranean strains of the Caucasian race. The persons in the present study with physical marks suggesting a kinship with the Mediterranean stock are, in general, most intense in their reactions of fear and anger; below them come those with Nordic marks; while still less intense emotional reactions were displayed by persons whose bodily marks are Alpine.

(7) Even less intense reactions of fear and anger than those of our 'Alpines,' however, occurred in a group whose bodily marks do not connect them clearly with any of the three strains more commonly recognized. This nameless group is marked by a cephalic index of 78-80 alone, or by this mesocephaly conjoined with dark hair or dark eyes or both. This group in its physical marks is closest to the 'Mediterraneans' of the present study. And yet in the intensity of emotional response it is farthest from these very 'Mediterraneans.'

(8) The difference in emotion here connected with variation in shape of head, color of eye, and shade of hair is probably not due to an accidental concurrence of the bodily variation with some systematic difference in the physical or social environment of the persons here studied. The present writer inclines to regard the emotional differences not as acquired, but as innate; as due probably to an unobserved difference in the psychophysical constitution of the groups here compared.

THE INFLUENCE OF THE REPETITION OF A SITUATION

By EDWARD L. THORNDIKE, Columbia University

In the *Fundamentals of Learning* we presented evidence from a variety of experiments that the mere repetition of a situation did not result in the strengthening of those responses which were made to it frequently at the expense of those which were made rarely. Except for some force other than differences in frequency among the responses to it, these responses will remain the same at the end of a thousand repetitions as at the beginning. This is not because the connections $S \rightarrow R_1$, $S \rightarrow R_2$, $S \rightarrow R_3$, etc., are unmodifiable and maintain rigidly the exact relative strengths which they initially display. On the contrary they may be, and in some of our experiments were, influenced notably by satisfying after-effects. A connection with an initial frequency of 1 in 10 may thus quickly attain a frequency of 9 in 10. Also there were notable shifts in relative frequency apart from any obvious differences in after-effects, which showed that the connections in question were sensitive enough. Not their fixedness, but the impotence of differences in relative frequency to strengthen the more frequent at the expense of the less frequent was to blame.

The fact seems of great importance, first because, if generally true, it makes allowance for relative frequencies unnecessary in many experiments on learning, permitting us to assume that, except for some force other than the relative frequencies themselves, these will remain unchanged. In the second place, it amounts to a disproof of hypotheses of inhibition by drainage wherever these rely on the greater strength of the drainer over the drainee. In the third place, it makes all theories that explain adaptive modifiability as a consequence of the displacement of infrequent or weak by frequent or strong tendencies unsound at their very base.

An experiment devised primarily for an entirely different purpose provides material for a useful study of the influence (rather, of the lack of influence) of the relative frequencies of occurrence of the connections $S \rightarrow R_1$, $S \rightarrow R_2$, $S \rightarrow R_3$, etc.

In this experiment, *E* says a word, and *S* responds by saying any number from 1 to 6, and also any letter of his choice. *E* then says "Right" or "Wrong," these

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TABLE I

THE FREQUENCIES OF CHOICE OF THE LETTERS IN THE FIRST (F) 800 AND LAST (L) 800 CHOICES OF 4800 BY 24 Ss. ALSO THE FREQUENCIES OF THE FOUR LETTERS CHOSEN MOST FREQUENTLY IN THE FIRST 800 (4a), THOSE OF THE FOUR CHOSEN NEXT MOST FREQUENTLY (4b), THOSE OF THE FOUR CHOSEN NEXT MOST FREQUENTLY (4c), THOSE OF THE FOUR CHOSEN NEXT MOST FREQUENTLY (4d), AND THOSE OF THE TEN LETTERS CHOSEN LEAST FREQUENTLY IN THE FIRST 800 CHOICES (10).

Letter	1		2		3		4		5		6		7		8	
	F	L	F	L	F	L	F	L	F	L	F	L	F	L	F	L
a	111	37	51	21	92	49	52	55	41	70	146	105	75	80	38	29
b	27	166	55	57	35	14	28	75	56	35	82	97	34	50	64	56
c	14	4	38	18	36	66	81	11	87	75	60	15	19	35	82	85
d	29	0	25	36	91	60	68	19	66	44	60	46	57	31	52	47
e	60	94	14	2	53	98	43	8	48	100	7	1	45	32	69	47
f	7	0	41	14	29	22	28	5	26	28	28	15	26	25	10	34
g	6	2	51	24	36	63	57	55	13	42	17	1	28	27	37	21
h	6	2	31	4	4	0	24	34	9	22	4	1	17	53	7	7
i	58	1	18	0	13	4	6	0	7	13	2	0	32	18	23	4
j	30	71	15	0	9	2	0	0	0	18	9	3	2	12	4	0
k	50	22	90	109	12	23	3	0	11	9	0	0	65	7	21	5
l	57	64	53	3	49	52	70	100	49	4	17	13	48	51	30	5
m	13	1	91	62	57	37	30	99	36	32	35	5	25	31	22	83
n	72	81	6	0	27	9	66	66	34	16	7	126	24	11	29	34
o	78	102	1	1	13	56	5	1	31	28	5	3	40	27	42	36
p	13	3	12	20	19	22	5	0	52	39	48	12	26	65	59	81
q	2	10	3	30	1	9	1	1	2	6	18	16	9	17	18	2
r	76	4	62	124	45	52	43	88	36	17	95	29	91	8	50	84
s	16	0	36	72	45	70	64	48	80	75	6	3	53	39	27	48
t	56	123	24	45	30	65	12	1	33	15	4	3	17	47	51	16
u	17	2	2	0	20	3	24	1	8	27	0	3	12	10	18	10
v	0	1	5	68	31	20	2	0	32	5	0	3	19	12	6	8
w	0	0	4	0	11	0	1	0	8	10	1	1	2	17	13	1
x	0	10	56	85	12	0	41	88	6	41	64	109	18	49	2	3
y	2	0	8	2	7	0	44	44	26	26	70	158	4	22	24	54
z	0	0	8	3	23	4	2	1	3	3	15	32	12	16	2	0
4a	337	224	299	380	293	244	285	196	289	229	393	389	288	134	274	269
4b	231	282	210	105	175	237	217	202	190	213	232	182	180	172	195	183
4c	136	259	146	108	132	165	157	283	138	70	98	37	126	97	134	89
4d	60	7	82	81	99	38	104	115	116	97	48	49	94	142	96	189
10	36	28	63	126	101	116	37	4	67	191	29	143	112	255	101	70

announcements applying to the *numbers only*. *S* is told that no letter is ever right or wrong, that they have nothing whatever to do with the learning, but are for use in a different problem, and that, whereas he receives a money payment for each right number given by him, he will receive just the same no matter what letters he says. He is asked, however, not to repeat the same letter, nor to follow any system (such as saying a, b, c, d, a, b, c, d, a, b, c, d, etc., in order). The words said by *E* are in 24 sets of 40, each set being repeated 5 times before going on to the next set.

We thus have for each *S* 4800 choices of a letter in response to the situation whose

TABLE I (*Continued*)

Letter	9		10		11		12		13		14		15		16	
	F	L	F	L	F	L	F	L	F	L	F	L	F	L	F	L
a	162	152	125	46	67	25	0	69	123	68	7	143	119	202	62	34
b	150	89	82	69	14	28	37	51	24	0	66	77	18	17	43	53
c	16	40	142	32	49	4	18	5	89	115	32	104	22	6	54	42
d	1	1	124	77	48	32	19	44	16	8	11	72	47	11	37	28
e	4	95	82	50	21	25	4	21	85	108	2	85	53	74	28	12
f	91	123	19	87	12	7	85	64	3	0	30	38	16	4	22	21
g	138	112	75	62	19	26	13	45	23	23	101	56	27	4	20	14
h	33	2	74	42	8	13	11	20	15	0	7	88	33	1	39	7
i	9	1	0	0	58	18	1	0	9	1	2	10	17	83	0	14
j	0	0	0	66	1	45	11	3	1	0	36	0	5	1	4	0
k	15	1	0	13	3	37	54	53	0	0	1	0	19	11	30	10
l	22	130	0	45	67	31	168	67	68	107	5	0	71	30	70	62
m	26	4	4	2	14	2	130	64	18	6	122	117	45	10	31	45
n	13	12	12	11	79	119	24	81	15	2	23	4	56	27	18	57
o	5	4	9	21	61	49	35	28	95	186	6	0	24	90	56	34
p	3	0	12	3	10	8	22	14	7	0	132	3	22	53	38	70
q	17	0	1	16	32	1	11	1	0	0	45	0	5	0	43	6
r	25	33	0	0	51	44	45	22	99	58	106	1	38	39	22	29
s	5	0	0	53	40	84	42	21	11	7	6	0	49	32	23	74
t	8	0	0	0	77	78	53	84	9	0	3	0	35	6	22	39
u	1	0	0	0	1	72	1	3	2	0	1	0	25	65	1	14
v	1	0	2	4	1	6	8	28	1	0	0	0	6	1	13	13
w	33	1	0	0	0	2	0	8	1	0	0	0	5	0	1	16
x	8	0	6	0	55	17	2	2	32	71	49	2	2	28	83	75
y	0	0	1	1	7	21	5	2	52	40	5	0	20	5	5	9
z	4	0	30	100	5	6	1	0	2	0	2	0	21	0	35	22
4a	551	476	473	224	290	253	437	248	402	420	461	177	299	333	271	205
4b	117	40	261	254	225	128	177	178	241	333	196	79	179	92	179	108
4c	70	171	52	122	169	121	100	167	81	37	96	218	117	162	140	130
4d	38	13	13	22	68	81	53	73	50	10	26	231	92	63	104	152
10	24	100	1	178	48	217	33	134	26	0	21	95	113	150	106	205

main component is "Say a letter" the minor components being the different words said by the *E* and the numbers said by *S* and whatever other stimuli may be acting.

Table I records the frequencies of choice of each letter by each of the 24 *S*s in their first 800 choices and in their last 800 choices of the 4800. Table I also records for each *S* the total frequencies in the first 800 choices for the four most frequent, the four next most frequent, the four next most frequent, and the four next most frequent, and for the ten least frequent; and the same facts for the last 800 choices. There is no waxing of the initially strong tendencies at the expense of the initially weak. On the contrary, 20 of the 24 *S*s show the opposite. In 19 of the *S*s the ten letters of least initial frequency gain.

TABLE I (*Concluded*)

Letter	17		18		19		20		21		22		23		24	
	F	L	F	L	F	L	F	L	F	L	F	L	F	L	F	L
a	15	65	40	0	90	82	170	20	136	32	192	31	46	54	27	42
b	91	104	35	121	87	64	58	47	135	76	136	72	51	71	88	115
c	32	49	62	1	87	53	25	23	44	11	57	5	30	87	23	106
d	17	15	59	106	21	95	37	34	98	34	11	7	131	57	20	22
e	15	53	0	0	6	16	148	77	104	47	8	7	54	70	7	1
f	16	12	83	63	16	20	33	19	18	5	17	1	29	62	28	31
g	16	10	1	0	2	3	14	30	69	102	6	11	22	43	16	0
h	38	7	0	1	28	1	9	68	43	34	11	72	55	30	26	3
i	18	38	0	0	34	9	0	11	36	2	0	0	20	48	11	1
j	9	0	0	0	20	17	0	11	4	11	0	0	1	0	68	74
k	11	0	0	0	7	20	5	53	0	73	1	85	96	45	8	2
l	125	64	104	125	0	0	81	79	7	66	16	6	65	47	72	123
m	9	19	75	99	34	8	49	66	11	71	29	55	31	45	43	82
n	9	44	28	110	104	151	0	1	7	14	7	27	28	10	22	5
o	21	13	1	0	97	68	0	2	10	41	2	64	29	29	8	33
p	22	3	0	0	4	7	12	68	3	1	16	144	9	4	16	0
q	3	1	0	0	6	3	12	5	5	9	0	0	3	2	68	12
r	20	22	46	1	16	1	3	3	16	68	125	50	24	26	51	31
s	44	37	73	145	27	122	2	60	8	42	117	112	5	3	42	0
t	44	22	133	20	7	10	39	27	13	2	8	9	7	38	14	6
u	1	75	1	0	0	0	0	2	1	3	0	0	7	4	9	0
v	6	3	1	2	0	0	1	12	0	6	0	1	1	7	31	46
w	29	8	0	1	9	11	0	40	0	1	0	0	3	5	8	2
x	52	77	1	0	74	30	59	7	9	28	8	41	30	4	46	60
y	80	59	0	0	17	0	43	5	8	15	1	0	8	11	10	0
z	57	0	57	5	7	9	0	30	9	6	32	0	18	0	38	3
4a	353	227	395	207	378	365	458	183	473	189	570	265	347	179	296	324
4b	178	143	251	257	229	100	187	152	192	149	135	61	182	240	182	173
4c	104	73	149	232	96	235	111	99	61	185	54	229	118	182	124	122
4d	71	87	4	2	58	32	38	194	39	78	31	84	94	127	91	216
10	90	270	1	2	39	68	6	172	35	199	10	161	59	74	107	45

Inspection of the table will convince the reader that the failure of the frequent to drain off strength from the infrequent is not due to the differences in frequency being small and due to mere chance. They are very large. In every *S* the four initially most frequent averaged over four times as frequent as the initially least frequent fourteen. In 17 *S*s the average initial frequency of the initially most frequent four was over ten times that of the ten initially least frequent.

We may, subject to the validity of one assumption, use the changes in the frequencies of the numbers 1 to 6 just as we have used the changes in the frequencies of the letters. The assumption is that the amount of

influence of an after-effect in the form of announcements of "Right" and "Wrong" is proportional to the number of times it occurs. The number of rewards (and of punishments) associated with any number is proportional to the frequency with which that number is chosen, since the six numbers were equally often "Rights." The six numbers will then have, except for chance, the same percentage of rewards, and, if the assumption

TABLE II

THE FREQUENCY OF CHOICE OF EACH NUMBER BY EVERY S IN THE FIRST 400 RESPONSES (F) AND IN THE LAST 400 (L) OF 4800

Number	1		2		3		4		5		6		7		8	
	F	L	F	L	F	L	F	L	F	L	F	L	F	L	F	L
1	22	59	59	49	70	65	101	62	79	49	41	64	164	108	42	97
2	47	59	66	65	61	77	65	88	102	90	71	92	139	56	103	91
3	63	58	90	90	71	82	115	78	73	84	127	80	31	67	125	58
4	110	104	60	57	76	74	65	60	105	82	49	76	32	63	44	43
5	80	87	80	77	68	55	27	46	30	63	76	44	21	40	55	79
6	78	33	45	62	54	47	27	66	11	32	36	44	13	65	31	32

	9		10		11		12		13		14		15		16	
	F	L	F	L	F	L	F	L	F	L	F	L	F	L	F	L
1	46	81	61	99	72	98	22	91	26	73	48	64	39	46	47	75
2	68	62	63	83	118	51	125	83	120	79	93	92	58	64	90	62
3	75	83	85	50	83	58	109	64	93	93	96	74	99	82	70	70
4	62	60	99	85	58	61	87	55	81	66	71	65	67	66	78	85
5	80	80	31	48	48	65	31	54	29	52	54	59	37	77	67	55
6	69	34	61	35	21	67	26	53	51	37	38	46	100	65	48	53

	17		18		19		20		21		22		23		24	
	F	L	F	L	F	L	F	L	F	L	F	L	F	L	F	L
1	48	59	89	49	11	72	75	100	48	52	28	65	57	109	47	77
2	95	66	48	93	49	64	80	105	32	65	61	70	84	116	102	70
3	86	65	78	78	156	90	59	36	71	82	126	101	80	78	71	54
4	73	73	35	71	117	75	80	38	86	71	78	51	73	77	97	68
5	49	75	64	56	41	56	65	55	107	88	70	67	52	7	54	71
6	49	62	86	53	26	43	41	66	56	42	37	46	54	3	29	60

is valid, the strengthening by "Right" (and weakening by "Wrong," if such occurred) would, except for chance, have the same proportions for 1, 2, 3, 4, 5, and 6 as their relative frequencies had. There is no reason to doubt this, though it is conceivable that a "Right" might have more influence upon the general tendency to choose the number in the first n occurrences (say, five) than 1/K times its influence in Kn occurrences (say fifteen).

It is also conceivable, and somewhat more probable, that the gross number of rewards had undue impressiveness, swamping that of the rewards for less frequently chosen numbers. This would work in favor of the waxing of the more frequent at the expense of the less frequent.

Table II shows the frequency of choices of each number by every *S* in his

first 400 and last 400 choices. The initially frequent do not gain. Taking for each person the sum of the two numbers most frequently chosen in the first 400, we find a loss in 20 of the 24 *S*s. The two that were least frequently chosen initially gain in 22 of the 24 *S*s. There is naturally not the enormous difference in frequency here that was found for the choices of letters, but the ratio of the average of the two initially most frequent to the average of the two initially least frequent is over 3.0 in 9 *S*s and over 2.0 in 17. These differences in frequency are large enough to cause substantial drainage from the weak to the strong in the intervening 4000 choices if any differences could cause such a drainage.

In the case of both the letters and the numbers, the initial tendencies do not remain stable (within the limits of chance) but the weak (or infrequent) tend significantly to gain at the expense of the strong. This is a matter worth investigating and I shall return to it later.

The next set of data are derived from the choices amongst four positions on a line (1 = left end; 2 = next to left end; 3 = next to right end; 4 = right end), among the choices neither rewarded nor punished in an experiment of Lorge and Thorndike in which the responses to some lines were rewarded when right and punished when wrong, whereas the responses to other lines were never rewarded or punished. Among the lines subject to reward and punishment, each position was equally likely to be rewarded.

For each *S* we have 2548 choices which, as recorded, fall into five groups of 512, 512, 512, 504, 508 respectively in order of time. For convenience we have raised the last two groups to equivalence with the former by multiplying their entries by 1.013 and 1.008.

Table III records the results for the 9 *S*s of the experiment. The most frequent in choices 1 to 512 lost in every set of 512 thereafter in 8 *S*s and in three out of four sets in the ninth. The most frequent in choices 1 to 1024, gained in the next 1024 in 5 *S*s (just barely in one of them) and lost in 4. In the last 1524 (raised in Table III to 1536) following, the most frequent in choices 1 to 1024 gained in 2 *S*s, lost in 6 and just barely lost in one.

In a similar experiment, but with records involving 1760 choices by each of 10 *S*s, the most frequent choice in the first 176, or the first 352, or the first 704 did not show a gain in the last 176, or 352 or 704. On the contrary it showed a loss in 9, 9, and 8 *S*s, respectively, by these three computations.

In all these experiments there is not an iota of evidence that the mere frequent connection by its frequency (or the stronger connection by its

TABLE III

THE FREQUENCIES OF CHOICES OF 1, 2, 3, AND 4 IN SUCCESSIVE FIFTHS OF 2548 CHOICES

S	Choice	Frequencies					
		1–512	513–1024	1025–1536	1537 to 2044× 1.013	2045 to 2548× 1.008	1–1024
I	1	145	166	140	184	195	311
	2	131	125	136	123	120	256
	3	157	137	139	128	125	294
	4	79	84	97	77	72	163
II	1	89	90	104	105	98	179
	2	169	149	161	148	152	318
	3	187	165	139	156	157	352
	4	67	108	108	103	115	175
III	1	130	138	154	165	140	268
	2	115	141	101	121	129	256
	3	160	134	141	124	122	294
	4	97	99	116	102	131	196
IV	1	125	161	157	131	136	286
	2	100	102	104	112	114	202
	3	146	114	110	137	128	260
	4	141	135	141	132	134	276
V	1	84	65	126	122	76	149
	2	199	161	200	183	140	360
	3	146	179	111	128	190	325
	4	83	107	75	79	106	190
VI	1	122	147	138	143	121	269
	2	132	93	97	109	106	225
	3	156	107	113	105	160	263
	4	102	165	164	155	123	267
VII	1	118	183	172	122	118	301
	2	111	131	153	147	137	242
	3	136	112	117	124	147	248
	4	147	86	70	119	110	233
VIII	1	156	100	116	124	120	256
	2	120	131	119	110	104	251
	3	148	152	165	165	155	300
	4	88	129	112	113	133	217
IX	1	90	124	117	136	138	214
	2	112	138	163	162	129	250
	3	154	136	133	123	151	290
	4	154	114	99	91	93	268

strength) waxes at the expense of the less frequent (or weaker). The departures from the status quo are all in the opposite direction of strengthening of the less frequent at the expense of the more frequent.

Although it is not the purpose of this article to explain the shifts in

choices, and additional experiments would be required to do so with surety, certain facts may be noted.

A certain fraction of them is, of course, statistical, due to the fact that the initially most frequent are so to some extent by mere chance, and will by chance regress toward the average status. This fraction is very small in cases where the number of choices taken as a sample of initial status is 400 or more.

A certain fraction of them may be due to a refractory period or phase developing from very frequent operation of a connection. This can hardly explain all, however, since the early and late choices were days apart.

A certain fraction of them may be due to a slight satisfaction in the form of relief when a less used response is given in the place of one that has become slightly annoying by its monotony.

A certain fraction of them, especially in the choices among the 26 letters, is due to the fact that the choices of letters are partly determined by ideas or short-lived habits of one sort or another and that these ideas and habits change from time to time in various ways. It is interesting to observe that the choices among the 26 letters diverged significantly from chance in all of the 24 *S*s. Four of the 26 letters had almost or quite 10 twenty-sixths of the first 800 choices in every *S*. In every *S* but one,[1] some letter had twice its chance frequency (31-) in the first 800 choices, and some letter less than one-tenth of its chance frequency.

Among the causes which made *S* have habits of preference, some may, and probably were, more or less temporary in their action. For example, some *S*s doubtless were influenced in their choices of letters by some characteristic of the word, such as its initial letter, or final letter, or dominant sound, or whatever in it caught attention. An acquisition or abandonment of such a habit or a change in it, could obviously cause notable shifts in the frequency of choices.

In the case of the choices of numbers, if, for any reason, an *S* got the notion that he was more often right when he chose a certain number, there would be satisfaction in choosing it which could strengthen the tendency. If he came to distrust this notion, there would be cause for a new shift.

In ordinary life, mental connections are under a powerful and pervasive control from their after-effects. Satisfactory after-effects tend to cause continuance of a connection if the situation continues. If it vanishes, they

[1] In that *S*, the frequencies in the *last* 800 choices did diverge enormously from chance.

still cause a strengthening of it, *i.e.* an increased probability of recurrence of it when the situation recurs. Annoying after-effects often cause the subject to change to a different connection if the situation continues. So there is progress toward a status of satisfaction, and stability when it is reached. When any one connection is as satisfactory as any other the brain's choice amongst them depends upon causes about which little is known, because such choices have been so little studied. They are well worth study.

Two things we know about them. They are sensitive and shift notably; their shifts are not in the direction of strengthening the more frequent or strong at the expense of the less frequent or weak.

THE SUSCEPTIBILITY OF PROSE TO RETROACTIVE INHIBITION

By JOHN A. McGEOCH and FRED McKINNEY, University of Missouri

The generality of the hypothesis that the phenomena of forgetting are to a large degree functions of interpolated events needs to be tested upon a wide sampling of materials.[1] Analysis of results from different materials may, moreover, be expected to shed light upon the conditions of retroactive inhibition. The present paper reports an experiment designed to measure the susceptibility of two different sections of meaningful prose to inhibition from the interpolated learning of other meaningful prose. In an effort to extend our knowledge of the character of the phenomenon of retroactive inhibition, itself a second condition of interpolation, the learning of a list of nonsense syllables, has also been included.

The prose paragraphs used in both of the experiments to be reported were written specifically for the purposes of these experiments and were based upon the information provided in Chapters 1 and 2 in Garrett's *Great Experiments in Psychology*. Two long sections, one on types of tests and the other on individual differences in intelligence, were prepared in such a way that, when divided in halves in terms of number of lines, each half contained 20 specific facts or ideas. The first half was used in each case as the material for original learning and the second half as the material for interpolated learning. The general theme is, thus, the same in both original and interpolated materials. The *Ss* were instructed to learn the facts and ideas of each material as completely as possible in the time given. The amount learned was tested by the answers to 20 questions, each of which was worded in a way to avoid any suggestion of the answer and at the same time to demand specific recall of one of the 20 specific items in the material studied.[2] The scoring was, save for a few answers which seemed to warrant partial credit, either right or wrong. The total score possible is, thus, 20 in each case. In the condition which employed nonsense syllables as interpolated material lists of 20 syllables of mixed

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[1] One of the writers has elsewhere brought together evidence in support of the major importance of interpolated events as determiners of forgetting. See J. A. McGeoch, Forgetting and the law of disuse, *Psychol. Rev.*, 39, 1932, 352-370.

[2] The two following questions are representative: (A) What percentage of the population is feebleminded? (B) What was the average Alpha score made by white enlisted men?

associative value were studied for the same length of time as was the prose, but were recalled, without questions, by the method of retained members.

Both experiments have been conducted with groups of *Ss* and, in order to prevent the *Ss* from copying from each other, one section of prose was used with those in the odd-numbered seats and another with those in the even-numbered seats.[3] This precaution, together with the careful watch maintained by *E* during the writing of the recalls, has reduced the variable of copying very nearly to zero. Each section was presented to *S* mimeographed on a single sheet and 5 min. were allowed for study. Immediately after this period *S* wrote his answers to the 20 questions for which 5 min. were allowed. After an interval of 15 min. from the end of this recall *S* wrote a second set of answers to the same questions for which 5 min. were allowed. One week later, at the same hour, a third 5-min. recall was written in answer to the same questions. Exactly the same procedure was followed when the nonsense syllables were employed as the materials for interpolated learning, except that learning was tested by the method of retained members under instruction to write down as many of the syllables as could be recalled. The general procedure was a constant for all groups and under all conditions.

The variables under experimental control are, of course, the interpolated events which fill the first 15-min. interval. Under the rest or control condition the *Ss* took the Seashore pitch discrimination test for 15 min., writing their judgments on a previously prepared record sheet. This is far from a "rest" condition in the sense of freedom from mental activity, but it requires no learning activity, the variable which we wish to eliminate during this interval, and some device must be used to prevent untrained *Ss* from rehearsing. Under the first work condition the *Ss* began to study the interpolated section of prose, either II or IV, immediately following the completion of the recall of the original section. They studied this interpolated section for 5 min., immediately wrote during the next 5 min. their answers to the 20 questions about it, and then spent the remaining 5 min. taking the Seashore pitch discrimination test. Under the second work condition, the *Ss* began to study 20 nonsense syllables in two lists of 10 each immediately after the first recall of the original material, under instruction to study the first list for 2 min., then at a signal from *E* to begin the second list, study it for 2 min., and finally at a second signal to spend 1 min. reading over the two lists together. After 5 min. of study, he spent 5 min. in recall and then 5 min. taking the Seashore test. These two conditions differ from the rest condition only in the interpolated learning and recall, while they differ from each other only in the character of the interpolated material.

Three different groups of *Ss* were employed, one upon each condition, and the alternate members of each group learned different sections of the material under otherwise identical conditions. At the beginning of the experiment each *S* was given all of the material required by the experiment, including in the case of the second work condition blank sheets for the written recalls of the interpolated nonsense syllables. The materials were clipped together in order, with the proper designation written on the back of each. At each successive step in the experiment *S* had only

[3] The sections on types of tests are numbered I and II, those on individual differences are numbered III and IV. Sections I and III were learned as original materials and Sections II and IV as interpolated materials.

to place the sheet at which he had been working face down at the bottom of his packet of material, turn over the next sheet at the proper signal and proceed. It was possible in this way to adhere exactly to the time schedule described. At the end of the second recall the *Ss* were given detailed and rigid instructions against rehearsal. All *Ss* were students in General Psychology. The experiment was conducted early in the semester and the relatively technical material of the prose sections was unknown to them.

The procedure just described was followed in both experiments except for one difference which distinguishes the two from each other. In Experiment I recalls were taken of the original material, as described, immediately, after 15 min., and after 7 days. In Experiment II the 15-min. recall was omitted and only the recalls immediately and after 7 days were taken. It is, thus, possible to discover whether the distributed practice afforded by the recall after 15 min. has affected the results after 7 days. The procedure used in these experiments parallels exactly, except for the difference in material, a prior experiment upon the susceptibility of poetry to retroactive inhibition.[4] The report of the present experiments is presented in as nearly as possible the same form in order to facilitate comparison of the results from these two kinds of meaningful material.

TABLE I

ITEMS CORRECTLY RECALLED: MEANS, SIGMAS, PERCENTAGES OF RECALL AND OF RETROACTIVE INHIBITION

Condition	N	Time of recall: Immed.	After 15 min.			After 7 days		
		Mean	Mean	%Rec.	%R.I.	Mean	%Rec.	%R.I.
			Section I					
Rest	37	13.77 ± .49	15.47 ± .47	112.3		13.70 ± .58	99.4	
Work (Prose)	29	14.03 ± .42	14.88 ± .42	106.0	05.6	11.79 ± .62	84.0	15.4
Work (N.S.)	36	14.80 ± .56	15.80 ± .48	106.7	04.9	13.76 ± .50	92.9	06.5
			Section III					
Rest	37	17.51 ± .38	19.00 ± .26	108.5		18.70 ± .30	106.7	
Work (Prose)	24	17.83 ± .41	18.19 ± .34	102.0	05.9	17.85 ± .46	100.1	06.1
Work (N.S.)	35	17.37 ± .30	18.65 ± .18	107.3	01.1	17.97 ± .27	103.4	03.0

Retroactive inhibition has been computed in terms of the percentages of recall after the two intervals. The percentage of recall is found by dividing the mean raw score at immediate recall into the corresponding score at each delayed recall. The percentage under each work condition is then subtracted from the percentage under rest and the difference divided by the percentage under rest to give the relative amount of retroactive inhibition.

RESULTS

Experiment I. The data for the two sections of prose will be presented separately. This permits a comparison of the results from two different

[4] J. A. McGeoch and F. McKinney, Retroactive inhibition in the learning of poetry, this JOURNAL, 46, 1934, 19-33.

samplings of the material and will lend increased significance to the conclusions. In Table I are given the mean numbers of items correctly recalled (or questions correctly answered), the sigmas of the means, the percentages of recall and of retroactive inhibition. Percentage of recall has been found after each interval with the mean at immediate recall, which is the measure of original learning, in the denominator.

Each set of three groups has immediate recalls which are as close together in magnitude as could be expected. Section I is somewhat more difficult than Section III. Both, however, yield recall scores after 15 min. which are higher than 100%, a fact which is readily understandable when one remembers that the recalls were in response to the same questions each time and that a single recall is an incomplete measure.[5] Each section of prose shows a small percentage of inhibition from the interpolated learning of an equal section of similar prose.[6]

The differences involved are not satisfactorily reliable, but the fact that the same difference appears with each section of prose and that differences of similar magnitude appear consistently in other parts of the experiment support the conclusion that the recall of the ideational content of a prose passage is susceptible to a small amount of inhibition from the learning of the content of a similar passage. This amount of inhibition becomes larger in the next recall after an interval of 7 days, and the increase is a function of the prose section, being 9.8% in Section I and but 0.2% in Section III.[7] The latter amount is inconsequential, and the former, while much larger, by no means represents certain evidence of an increase in amount of inhibition with increasing time.

When we turn to the results from the interpolated learning of 20 nonsense syllables, it can be seen at once that very nearly the same statements apply to them. The percentages of inhibition are less in every comparison from the syllables than from the prose, but the differences are not large. There is the same increase with time, although it is less pronounced in the case of Section I, so that the difference between the inhibition produced by the two conditions is here the largest of all. The interrelationships are highly complex and an assessment of their statistical validity would rest upon assumptions of equal complexity. If, however, we depend for assurance upon the consistency of the direction of the differences following

[5] Cf. W. Brown, To what extent is memory measured by a single recall? *J. Exper. Psychol.*, 6, 1923, 377-382.

[6] The interpolated sections of prose were learned slightly better in each case than were the original sections. The immediate recalls of the interpolated syllable lists varied between 6 and 8.

[7] The implications of the data are not greatly affected if we use the recall scores after 15 min. as the basis for computation of percentages of recall after 7 days.

each interpolation, we may conclude that the nonsense syllables have a small amount of inhibitory effect but one which is only a little less than that produced by the learning of the prose and one which parallels the variations of the latter with material and with interval.

Experiment II. This experiment is an exact reproduction of the first except that the second recall, after 15 min., has been omitted and that three new groups have been employed with each section of prose. It permits an estimate of the influence of this second recall upon the amount of inhibition after 7 days and provides an independent statement of the susceptibility of the two passages to inhibition. The results (Table II) show once more a consistently appearing but small amount of inhibition, although here the interpolated syllables produce a larger decrement in one case than does the interpolated prose. In conjunction with the other data this probably means only that the two interpolations are not far apart in the amount of their influence. Three of these percentages of retroaction are slightly larger than those at recall after 15 min. in Experiment I, and three are slightly smaller than the corresponding percentages after the 7-day interval of the first experiment. The two experiments yield evidence of a slight but statistically unimportant tendency for retroaction to increase in amount from 15 min. to 7 days and no evidence that the second recall, after 15 min., has had any marked influence upon the amounts of inhibition after 7 days.

TABLE II

ITEMS CORRECTLY RECALLED: MEANS, SIGMAS, PERCENTAGES OF RECALL AND OF RETROACTIVE INHIBITION

Condition	N	Time of recall			
		Immed.	After 7 days		
		Mean	Mean	%Rec.	% R.I.
		Section I			
Rest	27	14.44 ± .61	11.81 ± .68	81.8	
Work (Prose)	33	13.04 ± .47	9.80 ± .61	75.1	08.1
Work (N.S.)	33	13.69 ± .53	9.33 ± .66	68.1	16.7
		Section III			
Rest	21	16.11 ± .53	16.35 ± .42	101.4	
Work (Prose)	26	16.67 ± .45	16.14 ± .53	96.8	04.5
Work (N.S.)	32	16.62 ± .38	16.46 ± .41	99.0	02.3

DISCUSSION

The results of two experiments have uniformly shown that prose content is susceptible to a small degree of inhibition from the learning of a second section of prose of the same kind, that there is a slight but somewhat uniform tendency for inhibition to be larger after 7 days than after 15 min.,

and that there is no uniform variation in results following the omission of the recall after 15 min. The interpolated learning of 20 nonsense syllables is followed by a decrement which is only a little less than that which follows the interpolated prose. These results present three general questions for discussion. (*1*) How do these results compare with those from our earlier experiments upon the susceptibility of poetry to retroactive inhibition? (*2*) What is the importance of the fact that prose is susceptible, even though slightly, to a decrement from interpolated learning? (*3*) What is the significance of the fact that the interpolated learning of nonsense syllables has almost as large an inhibitory effect as does the learning of very similar prose content? We shall consider these questions in order.

(*1*) The present experiments have been performed under conditions which directly parallel those employed in the experiments with poetry and the results may, therefore, be directly compared. In the earlier experiments percentages of inhibition were found in terms of number of lines correctly recalled, number of which the meaning was correct, and number of which the meaning was only partially correct. The present experiment yields only a single score, the number of questions correctly answered. This score is not the direct analogue of any of the three employed with the poetry, but it is most closely comparable to the number of lines correct in meaning, since the answers were not required, except when the nature of the question obviously demanded it, to be recalls of the wording of the passage but only of its meaning. The amount of inhibition found with the prose, when other prose is interpolated, is less than that found with poetry, when other poetry is interpolated and when the scoring is in terms of meaning correct or of meaning partially correct. The prose is roughly equal to the poetry in susceptibility to inhibition when the poetry is scored in terms of lines verbally correct. Since all scores must be considered in evaluating the results with the poetry and since the retroactive effect is in one sense the total of all, it must be concluded that prose is less susceptible to inhibition than is poetry under the conditions of these experiments. This is especially true after the 7-day interval and still more so when the recall after 15 min. has been omitted. The increase in retroaction following the omission of this recall with the poetry does not appear with the prose. It may also be concluded that the interpolated learning of nonsense syllables is followed by a smaller decrement in the recall of the prose than of the poetry.

It is probable that the differences between the results with the two different materials are in large part a function of the method of recall. The number of as-

sociative cues presented at the recall of the prose was much greater than the number offered in the case of the poetry. The prose recalls were in response to 20 individual questions, each one of which, although framed to avoid suggestion of the answer, would necessarily be connected with it. The poetry recalls were, on the other hand, given in response to the single stimulus of the name of the poem and the words of the instructions and, although each line recalled would be a cue to the next, each line partially forgotten would introduce a break in the direct sequence of cues and would to that extent render recall more difficult. This circumstance would account for the increase in retroaction with time interval as well as for its greater amount after 15 min., since the forgetting which followed the non-experimental interpolations of everyday experience and which appeared under the rest condition meant a progressive diminution of the correct sequential cues to the recall of the poetry, while such forgetting could have no influence upon the cues (questions) to the recall of the prose. The results of the two experiments are in complete agreement upon the question of the susceptibility of meaningful material to a decrement from interpolated learning. The differences in amount and in change of amount with interval are, according to the interpretation given, functions of differences intrinsic to the method of measuring the recalls of the two materials.

(*2*) We shall now turn to the second question, that of the meaning of the small amounts of inhibition found with the prose. They must be interpreted in the light of their general setting. The conditions of these experiments approach the conditions under which human beings actually learn and recall. The method of complete presentation with continuous reading is the one by which students usually study and by which meaningful material is read by anyone. The testing of learning and of delayed recall by answers to questions upon the material approximates the method of the examination or any situation in which one is asked to reproduce piece-meal the specific content of previously read material. We have sought in this way to test further the generality of the hypothesis that inhibition from interpolated learning is a basic condition of forgetting. If retroactive inhibition is a phenomenon which is found only in the verbatim reproductions and with the serially learned materials of the usual laboratory investigations, its significance is narrowly limited. If, on the other hand, it appears under conditions such as we have employed in these experiments, its generality is increased and the hypothesis that it is a major determiner of forgetting in extra-laboratory as well as in laboratory situations is supported. The fact of the smallness of the amount of this inhibition should not be allowed to weigh against its importance. The uniformity of the inhibition with two different sections of the material and in two separate experiments is strong evidence of its validity. That the amount should be small is but to be expected from the fact of the relatively gentle decline in curves of retention for prose as compared with those for disparate serial material.

The interpretation of the latter fact waits upon a more adequate understanding of the character of retroaction itself and, perhaps, of other phenomena as well.

(*3*) The fact that the interpolated learning of nonsense syllables is nearly as potent to inhibit the recall of prose as is the interpolation of closely similar prose raises a third question. We have discussed this in connection with the similar results of the experiments with poetry and that discussion need not be repeated. The problems raised by the results with the poetry are still more keenly emphasized by those of the present experiment in that the similarity between serially learned syllables and prose studied for its meaningful content is still less than that between the syllables and serially learned poetry. The conditions of retroaction and the mode of its operation are made to appear more important and puzzling still by these results.

Conclusions

(*1*) When a section of prose closely similar to the one used for original learning is employed as interpolated material, small amounts of retroactive inhibition appear in recall after 15 min. and after 7 days. The susceptibility to inhibition after the 7-day interval is not importantly affected by the recall after 15 min.

(*2*) The use of nonsense syllables as material for interpolated learning produces nearly as large an amount of inhibition as does the interpolation of closely similar prose.

(*3*) These results agree in direction and in implication with those of a prior experiment upon the susceptibility of poetry to a decrement from interpolated learning. The results of both experiments increase the generality of this decrement-phenomenon and give support to the theory that retroactive inhibition is a major condition of forgetting.

THE INFLUENCE OF INSTRUCTION AND CONTEXT UPON PERCEPTIVE SEARCH

By HOWARD L. KINGSLEY, Boston University

When the *O*s of a previously reported experiment on search[1] were instructed to find an obscure object in a presented picture, it was found that they frequently looked for something quite unlike the object named in the instruction. Here the object as derived from verbal instructions did not coincide with the perceived object. This inadequate formulation of the search-task occasioned delays, confusion, annoyance, and even failure to consummate the search, for it left *O* unprepared to recognize, when he saw it, the thing he had been instructed to find. Because of this discrepancy between the task as understood and the object actually perceived, further experiments were arranged in which the obscure object was to be visually presented with the instruction.[2] In order to secure comparable results, pictures were mounted (as before) on gray cardboard (9 x 12 in.). From an extra copy of each picture a small part was cut and mounted on a cardboard (2 in. sq.), bearing, without words or other context, an exact duplicate of some object to be found in the larger picture.

Experiment I. *O* was instructed as follows:

> At the 'ready' signal you are to fixate the center of the screen. At 'now' a small picture will be presented. Observe it carefully. It will be quickly withdrawn and then a large picture will replace it. You are to find in the large picture the object shown in the small picture. When you have found this object, press the button, and immediately report the total experience of the exposure-period.

The small picture was exposed for 2 sec. When the picture was seen by *O*, significant forms usually stood out at once, though sometimes the impression was at first confused and then "the meaning emerged gradually" (*B*), or "verbal kinaesthesis gave meaning to the visual experience" (*Br*). Verbal kinaesthesis was abundant. It served to name the object and sometimes it carried descriptive comment (*e.g.* "fish—under water"), or antici-

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[1] H. L. Kingsley, An experimental study of search, this JOURNAL, 44, 1932, 314-319.

[2] The writer acknowledges indebtedness to Dr. Ethel Bowman (*B*) and Miss Elizabeth Rutherford (*R*), members of the psychological staff, and to Theresa Berney (*Br*), Charlotte Towson (*T*) and Anna R. Meeks (*M*), major students in the psychological laboratory of Goucher College.

patory comments (*e.g.* "flag—easy to find," "must be a beach scene," "this picture will be in black and white").

Since the first perception served as an occasional instruction directing anticipation, self-instruction also was frequent; (*B*: "pay close attention," "hold the image if you can," and "look for it in any position;" *T*: "look for characteristics in this picture that will help identify it in the text;" *M*: "try to take in all details of the small picture").

During the interval (2 sec.) there was always for *R* a definite visual image of the object just seen. For the other *O*s there was sometimes a visual image, sometimes perception of the gray screen, apprehension of the experimental situation, or silent repetition of the name of the object perceived. For *T* the visual image faded quickly and the object was carried by kinaesthesis. Anticipation of the setting of the object in the large picture was frequently reported; *e.g.* "I thought she must be sitting on a beach. I expected to see a beach" (*Br*), "I tried to place the object in its natural surroundings before the exposure of the second picture" (*M*). Self-instruction frequently appeared in anticipation of the impending search. When a house was shown in the small picture, *Br* instructed herself to "Look in the distance; it will be in the distance."

As soon as the large picture was exposed *O* began to search for the duplicate of the insert. The reports generally confirm the results of the previous experiments with regard to visual exploration and discovery; but the visual preview was responsible for certain differences. These we must discuss.

(*1*) *Exploration.* While large or brightly colored parts were occasionally perceived at once, there were, as a rule, few clear perceptions attained prior to the discovery of the sought insert. "I did not notice anything else in the picture. All was a confused mass until I clearly saw the cow. Nothing in the foreground was clearly apprehended" (*Br*). Strain and pressures, particularly about the eyes, and the shifting visual patterns carried the searching. "The harder the search, the greater the strain" (*Br*). Verbal kinaesthesis frequently implied "no flag there," "can this be it?", and other comments.

Visual images frequently referred to the sought object. With *R* they were replicas of the perceptive patterns of the small picture, persisting throughout the search and disappearing on discovery. *B* and *Br* often reported no visual surrogates. *M* used auditory-visual imagery during the interval but lost it when she looked at the details of the large picture. *T* "tried to hold the visual image, which was clear at first, but faded," giving place to kinaesthesis.

The visual designation proved superior to the oral method for giving *O* a correct and usable anticipation. But perceptions are not wholly determined by stimulus pattern,[8] and an object which is a part of a complex situation is not quite the same thing as it is in isolation.

In the initial perceiving of the isolated figure, *O* frequently supplied an imaginal context. A fish hanging on a line was "vaguely filled in by supplying a man with a rod and reel" (*R*), and a woman in a bathing suit with a striped parasol was supplied with a "visual background which meant *beach*" (*B*). Since the object sought was thus in part determined by imaginal factors contributed by the individual *O* it did not always match the object sought in the large picture. When it was at variance with the realized context it tended to delay or hinder discovery. (*E.g.* a cut-out of a light fixture was completed in imaginal terms to become a wall fixture. *O* was then "set to discover a fixture on a wall." But in the larger picture *O* found the wall covered with filled bookcases and only later discovered the fixture suspended from the ceiling.) The effect of this context was the same whether the supplementing came with the cut-out or was later supplied by search during the brief interval or while exploring the large picture. (*E.g. Br,* looking for a trowel, sought it in the hand of some person in the picture, but finally, after misdirection, found it lying on the ground.)

(*2*) *Discovery.* When finally apprehended, the sought object is clear and focal and is viewed with relief. "The clearness of the thing sought brings relaxation," and "there was a feeling of relief and relaxation when I found it" (*Br*).

All *O*s declared it easier to find the objects in this experiment than in the experiment where they were designated orally. "It is easier and more definite to have two visual things to match than to have a visual and a verbal experience" (*B*). The following comparison was made by *M*; "When the object was merely named you had to create your own image. Here you know just what it will look like, in color, shape, and size. This gives one a different attitude toward the task. It requires less effort. The search-performances with verbal designation were somewhat more interesting than these because you didn't know just what you were going to see. You had then to take some distinguishing feature of the class to which the object belonged, such as the beak of a bird or the wattles of a turkey, and watch for it. In this experiment you look over the picture as a whole, concentrating on some such aspect as the size, color, or form, of what you have seen in the preliminary exposure." *R* commented that "discovery is more

[8] M. Bentley, Environment and context, this JOURNAL, 39, 1927, 54-61.

immediate and more easily accomplished in this experiment. There is no conflict between the visual image and the perception, as often happened in the previous experiment. Here just a small part of the object in the periphery of the field of vision leads one to glance at once to the object. There is less aimless wandering of the eyes."

Experiment II. The context. In Experiment I the thing sought was always an integral part of the larger picture, definitely related to the other parts. Because of these contextual relationships, *O* was able to anticipate to some extent the location of the object sought, or at least to eliminate certain sections of the picture as altogether unpromising. The task was somewhat like that of the huntsman looking for a deer in the forest. He does not waste his time looking for game in the tree-tops. The purpose of the present experiment was to study search where no clues could be obtained from the arrangement of the constituent parts of the field of search.

From 50 to 60 small pictures of miscellaneous objects were pasted in irregular order on each of 12 large cards (9 x 12 in.). Each small picture was a separate unit which bore no relation to the others, except that of spatial proximity. A duplicate of one of the 50 or 60 pictures appearing on each of these cards was pasted on a small card (2 x 2 in.). The instructions were the same as before. Search was now limited to the place of the previewed small picture in the large heterogeneous group. It is marked, when the large group is presented, by peripheral vision, quick shifts of fixation, pressures about the eyes and shoulders, persistence of the visual image of the object sought, scanty perception of other objects, clarity of the object when found, and announcement of discovery in verbal terms (usually "there"). The following is a typical report by *R* (small picture of a chicken). "Visual image. Upon large exposure the center was first fixated. Rapid shift of fixation to the left. The chick appeared in peripheral vision. Image disappeared. Further shift made it foveal. Clear perception. The eye-movements were rapid, with pressures about eyes and shoulders. The other objects were not separately apprehended as figures."

B substituted verbal tags for visual images. Sometimes fixation halted on a group of lines while verbal kinaesthesis labeled the clarified object. Sometimes an attitude "not there" or a verbal labeling and rejecting came.

The distinctive feature of search under these conditions was the rapid running over of many "rejected" objects with no clear perception but with the realization that they were not the object sought.

Experiment III. Search for numbers. In the two preceding experiments *O* knew the appearance of the sought object because she had seen a duplicate

of it. Here recognition was immediate, certain, and satisfying. Now if the visual object was so standardized and familiar as to leave no doubt that it was the visual counterpart of the verbal symbol, it would seem that the oral designation should be quite as adequate for immediate and certain recognition as the visual designation. To test this supposition printed numbers were now used for search-objects, and the one to be found was given orally by the experimenter.

Six exposure cards (9 x 10 in.) were used. On each were pasted from 50 to 55 numbers. These numbers ranged from 1 to 30, were ¼-in. in height, and were printed on small squares and scattered about on the large card in an irregular way. Some were upright, others tipped, and a few were lying on the side. The instructions were the same as before except that *O* was instructed that she would be asked to find on the large card a certain number to be spoken (*e.g.* "7" or "27") just before the exposure. Thirty-six numbers were used.

The auditory perception of the number was followed either by a visual image of the number, printed in black, and verbal (throat) kinaesthesis (*R*), or by a combined vocomotor-auditory image (*B*). With *B* verbal kinaesthesis played the dominant rôle in defining and sustaining the task. Search was carried on here with the same general plan as in Experiment II. But peripheral vision was less effective, and *O* made more use of self-instructions and of comments (*e.g.* "neglect all but one-digit numbers," "this number is not quite right"). The same promptness and certainty in recognition appeared as with the visual designation of the earlier experiments.

The context was the same here as in Experiment II. The large card gave no clues of the sought number until this number was apprehended.

Experiment IV. The time element.[4] This experiment was undertaken to determine whether the visual method of designating the search-object used in Experiments I and II makes for quicker discovery than the verbal descriptions used in the earlier experiments. The same materials and procedures were used. Ten untrained students, divided into two groups, served as subjects. Both the visual and the oral methods of designating the search-object were used. One group was given even-numbered cards and the other the odd-numbered. The oral method was the same as before save that here *S* was instructed what to find just before, instead of just after, the picture

[4] This experiment was conducted in the psychological laboratory of The College of Practical Arts and Letters of Boston University. The writer was assisted by Dorothy L. McAlister and is indebted to her for the time records.

was exposed. Haste in announcing the discovery was cautioned. By means of a stop-watch, *E* measured the time which elapsed between the beginning of the exposure-period and the announcement of discovery. Each *S* had six trials under the oral method and six under the visual method in each of two series A and B which corresponded to Experiments I and II—24 trials, or 240 for the 10 *S*s.

The search-times for all *S*s under the oral method in the A-series is 3.1 sec., while for the visual method in this series it is 2.8 sec. with significant m.v. The median time for the oral method, B-series, is 3.8 sec., while for the visual method, B-series, it is 3.4 sec. Thus it appears that the visual method made for somewhat quicker discovery in both series.

Moreover, search required more time where the context was irrelevant than in the series where clues were obtained from the context. Under the oral method the median time was 3.1 sec. for the A-series and 3.8 for the B-series. Under the visual method the median time for the A-series was 2.8 sec., while for the B-series it was 3.4 sec.

Conclusions. These experiments have confirmed the previous series relative to visual exploration in a search-performance. Search and apprehension follow the same general course where the object to be found was verbally described or was visually presented. But the visual method of designating the search-object gives *O* a more adequate preparation for discovery than does the verbal method, it affords a greater degree of confidence, makes the task easier, and insures prompt, complete, and certain recognition of the object. It leads also to a quicker discovery.

Printed numbers, designated orally, were recognized quite as immediately and with as much certainty as were the miscellaneous objects designated by the visual method.

When *O* is precisely shown what to find, there is a tendency to anticipate the forthcoming context. If this anticipation is approximately correct it facilitates discovery; if in error it retards.

Where the search-field is so structured that the search-object sustains natural relations to its surroundings, the context usually affords helpful suggestions concerning the probable place of the object; but where the context is a mass of unrelated items, no clues are provided by it and the search becomes a systematic survey of the field in which only objects similar to the object sought are clearly perceived. More time is required to find an object when its context is irrelevant.

THE INFLUENCE OF VISUAL AND AUDITORY PATTERNS ON TACTUAL RECOGNITION

By THEODORA M. ABEL, Sarah Lawrence College

The object of this experiment was to study the influence of visual and auditory modes of presenting stimuli on tactual discrimination, thus employing a different type of learning condition from the more usual requirements of memorizing nonsense or meaningful words or sentences.

In investigations on verbal learning the results have often been conflicting, some favoring visual, others auditory presentation methods.[1] Differences in procedure and in experimental task-conditions produce differences in results.[2] But interpretations of the results of a given experimenter frequently do not account for and often conflict with the explanations of another experimenter. For instance, the class-room lecture method favors auditory presentation of learning material for one group of undergraduates,[3] but apparently has no such influence on the learning ability of undergraduates in another experiment.[4] In the latter investigation, visual presentation facilitates the learning of nonsense syllables because of its longer stimulation time for the syllable as a whole.[5] This explanation, however, cannot account for experiments where oral presentation of stimuli has been preferred.

By introducing a task of a less verbal character, in which the possibilities of organization of meaning are more remote than in nonsense syllables, and which require of the *S* direct sensory cues under quite unfamiliar learning conditions, we hoped to reach a better comprehension of this problem, particularly in the direction of interpreting results.

The task was to learn to recognize by touch 12 letters of an alphabet arbitrarily set up but similar to the Braille point system for the blind. Various combinations of 2 to 6 dots were used. The dots were punched on heavy manila paper cards (1.5 in. x 5 in.) by the regular procedure of writing board, frame and stylus employed for writing in Braille, and were shellacked in order to preserve uniformity in size. The stimuli were placed in a covered cardboard box, and were felt through an aperture at the side of the box facing the *S*, large enough for the index finger of the right hand. The left side of the box was open where the cards protruded

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[1] For a recent bibliography on the influence of sensory modality, see John A. McGeoch, The psychology of human learning: A bibliography, ref. nos. 672-705, *Psychol. Bull.*, 30, 1933, 1-62.

[2] *E.g.* in employing the anticipation method of memorizing nonsense syllables, Koch found combined visual-auditory and visual presentations superior to auditory presentation (this JOURNAL, 42, 1930, 370-388). The stimuli were presented both visually and orally. On the other hand, Reed found auditory presentation superior to visual presentation in an experiment on paired associates (*J. Exper. Psychol.*, 14, 1931, 632-649). Here the nonsense syllable to be associated with a meaningful syllable was always presented visually.

[3] Reed, *op. cit.*, 643. [4] Koch, *op. cit.*, 374. [5] Koch, *op. cit.*, 384.

(3/4 in.), so that the *S* could slide a card to the back of the box when through with it, without seeing the dots.

Experiment I. The *Ss* were 170 college women, divided into groups of 85 each: a visual and an auditory group. The groups were equivalent in age and on the Thorndike aptitude ratings.[6] Before being presented with a tactual stimulus, the visual group (VP) was shown for 3 sec. a large placard, on the left of which was printed in India ink the name of the tactual stimulus to be felt, and on the right of which was indicated the pattern of the dots to be felt. Before being presented with the tactual stimulus, the auditory group (AP) listened to an auditory pattern of the arrangement of the dots, played by *E* on a wooden block xylophone (concealed from the *Ss*). Three notes, high, medium and low, an octave and the fifth between, were used. The *Ss* were uniformly trained to understand that a sequence from high to low meant a vertical sequence of dots, a sequence of two identical notes meant an horizontal sequence, and a sequence from low to high note meant a diagonal sequence of the dots from lower on the left to higher on the right. The notes were played at the rate of 1.5 per sec.

Two letters not in the series were practiced before the learning began. The learning procedure consisted of two learning trials and a recognition trial for the first 6 letters, followed by two learning trials and a recognition trial for the next 6 letters. Then came a learning trial of all 12 letters, a rest of 5 min., and a final recognition trial of the 12 letters. Five seconds were allowed for feeling each stimulus-card under both learning and recognition conditions. In the first learning trial for each of the sets of 6 letters, the order of presentation of the names of the stimuli was known by the *Ss* (the names being the first 12 odd letters of the alphabet, A-C-E, etc., through W). In all other trials the sequence of presentation was worked out in a chance order and was not known by the *Ss*. The experiments were performed in groups ranging from 5 to 15 *Ss* in number. Scoring was made on the basis of the number of correct recognitions; immediate recognition-scores comprising the results of the first two recognition-trials taken together, and mediate recognition scores comprising the results of the final recognition-trial.

Experiment II. Previously we had performed a similar experiment on another group of *Ss* with a different object in view. We found we could use some of the results of this experiment for comparative purposes and to serve as a control to Experiment I. Ninety-nine *Ss* learned to recognize another set of 12 tactual letters comparable in difficulty to the set in Experiment I.[7] Fifty of these *Ss*, designated as the auditory control group (AC), were told what letter they would feel, and 49 *Ss*,

[6] The ages ranged from 16 to 21 yr. with the average for the visual group at 18.42 ± .679 and for the auditory group at 18.73 ± .659. Thorndike ratings for 59 of the visual group averaged 65.431 ± 7.996 and for 56 of the auditory group averaged 63.227 ± 7.281. We did not have Thorndike ratings for the 55 students from Vassar College. They were equally distributed in the visual and auditory groups, however.

[7] Comparability in difficulty was estimated on the basis of the work of P. Villey, Psychologie de la lecture tactile, *J. de psychol.*, 28, 1931, 213-249, in which he indicated the tactual letters that were remembered more easily.

In Experiment II the names of the stimuli were the first 12 letters of the alphabet (A-L). This might possibly have made a slightly easier task in the recall of the names of the tactual stimuli. As only 2 *Ss* in Experiment I and 1 *S* in Experiment II made the error of assigning a name not in the series, we can assume that this difference was negligible.

the visual control group (VC), were shown on a large cardboard the name of the stimulus they would feel.[8] Neither group was presented with a pattern, visual or auditory, of the arrangement of the dots. Other conditions for the procedure of the experiment were the same as in Experiment I. As a longer interval had been allowed before the final recognition of the letters, only the scores of immediate recognition could be employed for comparative purposes.

Experiment II was conducted during the spring term of 1932 at Sarah Lawrence College. Experiment I was conducted during the fall term of 1933 at Sarah Lawrence College and at Vassar College.[9] The author acted as *E* in all experiments.

Table I gives the results of immediate recognition in Experiments I and II and for mediate recognition in Experiment I, and indicates the reliability of the differences between the averages of the various groups.

TABLE I

AVERAGE SCORES AND DIFFERENCES, EXPERIMENTS I AND II

Group	No. of Ss	Av. no. correct scores	
		Immed. recog.	Mediat. recog
AP	85	6.785 ± 1.949	5.529 ± 2.21
VP	85	8.317 ± 1.719	7.788 ± 1.831
AC	50	7.255 ± 2.051	
VC	49	7.019 ± 1.859	

Groups compared		Actual diff.	$P.E._{diff.}$	Chances in 100 for true diff. greater than zero
Immed. recog.	Mediat. recog.			
AP–VP		1.532	.282	100
	AP–VP	2.259	.313	100
AC–VC		.236	.979	55
AC–AP		.470	.366	81
VC–VP		1.298	.317	100

There was no reliable difference in the scores of the AP and of the two control groups. The number of letters correctly recognized for all 3 groups ranged from 6.8 to 7.2. The VP group did better, averaging slightly over 8 letters correct, or 17% better than the AP group. The actual difference between the average of the VP group and of the other 3 groups was reliable.[10]

Experiment III. In another experiment, following Experiment I by exactly 3 weeks to the hour for all *Ss* participating, 38 of the former VP group learned a new set of letters, comparable in difficulty to the first set by the auditory pattern method;[11] and 34 of the former AP group learned these letters by the visual pattern

[8] The age ranges were the same as those of the groups in Experiment I. The Thorndike ratings were also equivalent, the average for group VC was 65.789 ± 7.234 and for group AC 63.864 ± 7.1665.

[9] Acknowledgment is due Professor Josephine M. Gleason of the Psychology Department of Vassar College for allowing us to use her students in the introductory laboratory course as *Ss* in this experiment, and to Miss Mary Elizabeth Stanwood for making the stimulus-cards.

[10] An attempt at measuring the reliability of the task was made in correlating the first half with the second half of the mediate scores of VP and AP. The reliability *r* for VP was +0.64 ± .05 and for AP +0.48 ± .06 (product-moments method).

[11] See footnote 7.

method. The names of the stimuli were the first 12 even letters of the alphabet (B-D-F through X).

As can be seen from Table II, both groups improved by approximately 2 letters in efficiency, but again the VP group did 17% better than did the AP group, the difference being reliable.[12]

TABLE II

AVERAGE SCORES AND DIFFERENCES, EXPERIMENT III

Group	No. of *Ss*	Av. no. correct: Immed. recog.	Av. no. correct: Mediat. recog.
AP	38	8.184 ± 1.756	7.605 ± 2.282
VP	34	10.114 ± 1.551	9.286 ± 2.2099

Groups compared	Actual diff.	$P.E._{diff.}$	Chances in 100 for true diff. greater than zero
AP–VP (Immed. recog.)	1.93	.387	100
AP–VP (Mediat. recog.)	1.68	.512	98.7

There is indication that *Ss* who did well in Experiment III did well also in Experiment I. Using the rank correlation method, with conversion into Pearson coefficients, we found the correlation between AP_I and VP_{III} to be $+ 0.45 \pm .09$ and between VP_I and AP_{III} to be $+ 0.64 \pm .07$. This is in accord with the results of other experimenters which show that individuals who learn best by one mode of presentation of stimuli, learn best by another mode as well.[13]

The *Ss* who acted in Experiments I and II were asked to comment on the visual and auditory methods and to give any reasons that occurred to them for the success of one method over the other (in case one method was found easier). Of these *Ss*, 74% stated that the visual method was easiest: 57% gave positive reasons for their judgments, such as influence of training and habit on eye-hand manipulations, more meaning, better connections and more unification in the visual patterns for the tactual patterns; 17% gave negative reasons for their judgments of the superiority of the visual mode of presentation, such as "sounds too confusing or distracting," "the meaning of the sounds had to be created and required too much effort," or "the sound had to be translated into visual imagery before it made sense in terms of the touch patterns." On the other hand, 17% of the *Ss* expressed the opinion that the auditory method was the easiest because of the fact that they were trained musically and used "to getting" rhythms. Actually, only one-half of these *Ss* had a better score with the auditory method.

DISCUSSION OF RESULTS

In the specific task of learning to recognize by touch small dot patterns, the aid of a visual presentation of the pattern was more effective than the presentation of an auditory pattern. The presentation of the auditory pattern was neither more nor less effective than a no-pattern presentation, where the name of the tactual stimulus

[12] The actual difference between the average median scores is only 3 times the P.E. of the difference, but with the small number of cases this difference can be considered quite reliable.

[13] G. Bennett, The correlation between different memories, *J. Exper. Psychol.*, 1, 1916, 404-418.

was announced either visually or orally. The sense modality through which the name alone, without the pattern of the stimulus, was presented played only an insignificant rôle in the total learning process.

It seems fairly obvious that the more immediate coherence and significance of the visual patterns for the *Ss* determined the superior part played by vision in the comprehension and recognition of the tactual patterns. We have seen that the comments of the *Ss* indicate the nature of the help visual patterns gave them in interpreting tactual patterns. In fact, 34% of the *Ss* used the terms, "more meaning," "better connections," or "more unification" to characterize the influence of the visual patterns.

Since beginning this experiment we have found an instance in which auditory patterns were more immediately significant than visual patterns for learning in a particular task situation. In a report on visual *versus* auditory methods of teaching bi-manual production of rhythmic patterns, Schick[14] stated that one of the reasons the auditory method was superior to the visual in her learning problem, was because of the more immediate comprehension of the auditory rhythms, whereas the visual rhythms required the added difficulty of reading and interpreting notations.

Two factors, it would seem, contribute to the greater significance of the visual patterns in our experiment. The first factor is the influence of the constant integrations being made between touch and vision in the contacts an individual has with the world around him. As the individual develops from childhood to adulthood his tactual experiences become more and more dominated by vision, and he depends more and more on his keener visual discriminations to render more acute his tactual discriminations.

The second factor is the influence of the similarity between visual and tactual sensory organizations. In both fields the "primitive and natural" organizations have in common the quality of spatiality or extensity, so that a visual space pattern can more readily be translated into a tactual space pattern than can an auditory temporal pattern. Possibly we can carry over Lashley's interpretation of the "equivalence of stimuli" within a given sensory field to explain the similar pattern perceptions of two different sensory fields.[15] We may then say that between vision and touch, the patterns of stimuli are more nearly equivalent than between hearing and touch, in the sense that they have in common ratios of proportion in the spatial distribution of excited points.[16]

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[14] H. F. Schick, Proceed. 40th ann. meet. Amer. Psychol. Assoc., *Psychol. Bull.*, 29, 1932, 682-683.

[15] K. M. Lashley, Basic neural mechanisms in behavior, *Psychol. Rev.*, 37, 1930, 1-14.

[16] Lashley, *op. cit.*, 5.

THE COLOR PREFERENCES OF 1032 YOUNG CHILDREN

By THOMAS R. GARTH and ELECTA PENINA PORTER, University of Denver

Previous inquiries into the color preferences of young children have been made, but the groups were usually small so that generalizations are on that account somewhat precarious. Another inadequacy is found in the lack of numerical expressions of the relative "pulls" of the different colors. What is needed is a study of a group large enough to allow for individual differences in so far as it is possible, and a statement of the results in such form that comparisons can be made of a more or less quantitative character.

Studies of small groups have been made by Shinn[1] in 1907, Myers[2] in 1908, McDougall[3] in 1908, and Woolley[4] in 1909, but the results, here listed, are cramped by the above mentioned inadequacies. However, Shinn has shown that a color preference is exhibited in the second half year of the life of a child, lying at the red end of the spectrum. McDougall, working with children from the fifth month on, and using the method of paired comparisons, found that red, green, and blue are preferred to white. Myers found results coinciding with those of McDougall. Woolley experimented upon a girl infant almost 7 mo. old, and found, using the method of paired comparisons, the child's preference for red and an indifference to green. Blue and yellow held an intermediate place, with yellow slightly in the lead. The preference does not coincide with the brightness scale.

The largest group of young people afforded by the literature is that of 212 kindergartners. This is reported on by Dashiell[5] who in the same report gives comparative results between these kindergartners and a group of college sophomores. He found a striking similarity of preference between the groups of young and old *Ss*.

In the present experiment it is sought to obviate the errors which may come of using small groups and the lack of definiteness resulting from inadequate quantitative measures of the results. Results from a previous study[6] of 1000 older children are here used for purposes of comparison.[7]

Subjects. The *Ss* of this experiment were 1032 young children. These were divided into 3 groups: (I) 439 pre-school children; (II) 381 kindergarten children; and (III) 188 first-grade children. The ages of the children in Group I ranged from 1 to 4.9 yr.; in Group II from 4 to 6.9 yr.; and in Group III from 6 to 6.9 yr. These children were found in urban communities and rural sections in Colorado

* Accepted for publication January 2, 1933.

[1] M. Shinn, The development of the senses in childhood, *Univ. Calif., Pub. in Educ.*, 4, 1907.

[2] C. S. Myers, Some observations on the development of the colour sense, *Brit. Jour. Psychol.*, 2, 1908, 353-362.

[3] W. McDougall, An investigation of the colour sense of two infants, *Brit. Jour. Psychol.*, 2, 1908, 338-352.

[4] H. T. Woolley, Some experiments on the color perception of an infant and their interpretation, *Psychol. Rev.*, 16, 1909, 363-376.

[5] J. F. Dashiell, Children's senses of harmonies in colors and tones, *J. Exper. Psychol.*, 2, 1917, 366-475.

[6] T. R. Garth, A color preference scale for one thousand white children, *J. Exper. Psychol.*, 7, 1924, 233-241.

[7] Cf. also W. E. Walton, R. B. Guilford, and J. P. Guilford, Color preferences of 1279 university students, this JOURNAL, 45, 1933, 322-328.

and Arizona. The children of Group I and of some of the other groups were found in various settings, as in the home, Sunday School, and on playgrounds. All the rest were found in public and private schools.

Materials of experiment. The materials of the experiment were 7 color stimuli. Six of these were saturated colors as red, green, orange, violet, blue, yellow. The

TABLE I

SHOWING RANKS, SCALES, AND COEFFICIENT OF VARIABILITY

(Numbers in parentheses give the number in the group)

	Larger groups				Pre-school				
Colors	Rank	A Younger (1032)	Rank	B Older (1000)	Rank	C 1–2.9 yr. (62)	Rank	D 3–3.9 yr. (124)	Rank	E 4–4.9 yr. (128)
Red	1	185	3	122	3	298	1	138	1	208
Blue	2	174	1	204	6	136	1	138	2	204
Green	4	151	2	126	2	309	3	131	3	197
Orange	3	155	5	100	1	316	3	131	4	185
Violet	5	127	4	107	4	272	6	120	5	178
Yellow	6	105	6	69	5	147	5	124	6	125
White	7	0	7	0	7	0	7	0	7	0
C.V.		.15		.24				.03		.05

	Kindergarten				First Grade		Sex Groups			
Colors	Rank	F 5–5.9 yr. (102)	Rank	G 6–6.9 yr. (102)	Rank	H 6–6.9 yr. (126)	Rank	I Boys (495)	Rank	J Girls (537)
Red	2	109	1	308	3	179	1	199	2	189
Blue	5	141	2	207	1	262	2	169	1	193
Green	3	175	3	224	5	153	3	147	4	107
Orange	1	201	4	177	2	219	4	136	2	189
Violet	4	145	5	170	5	127	5	110	5	136
Yellow	6	100	6	120	6	105	6	91	6	114
White	7	0	7	0	7	0	7	0	7	0
C.V.		.12		.03		.24		.22		.16

seventh stimulus was the achromatic white. All these were on the Milton Bradley coated papers in the form of disks ½ in. in diam., which were mounted on small white cards. The 7 stimuli made a set and the sets were contained in small envelopes. The method of administration was the simultaneous presentation of the colors to a child with intent of having him pick out from the set the one he liked best, then from the 6 left, the one he liked best, and so on until he had only one left as the one he liked least of all. The process was more directly supervised in the case of the very young children, where the child was persuaded to make at least 3 arrangements of order of preference so as to insure that his real order of preference was obtained.

The scales. The rankings of the 1032 children were recorded in blocks with reference to age, sex, and school experience. In order to find some numerical value for the affective reaction to a color, scales were obtained in terms of D/MD, built upon an arbitrary zero value, *i.e.* white. (See Hillegas[8] for discussion of the method

[8] M. B. Hillegas, *A Scale for the Measurement of Quality of English Composition of Young People,* 1913, 18-25.

of obtaining a scale.) If a color is preferred to another color 25% of the time, the scale difference between them is 1.00; if 35%, it is 0.57. Table I gives the ranks of the colors, various scales, and coefficients of variability for values of the 6 colors above white. (See Table I for the scales and Fig. 1 for this graphic representation.) It will be seen that the scale for the 1032 young white children (Group A) is slightly different from that for 1000 white children, whose ages run from 6 to 19 yr., and who were in school grades 1 to 10 (Group B). In the case of the children having more wide-spread ages and grade experience, 1000 cases, the value of yellow

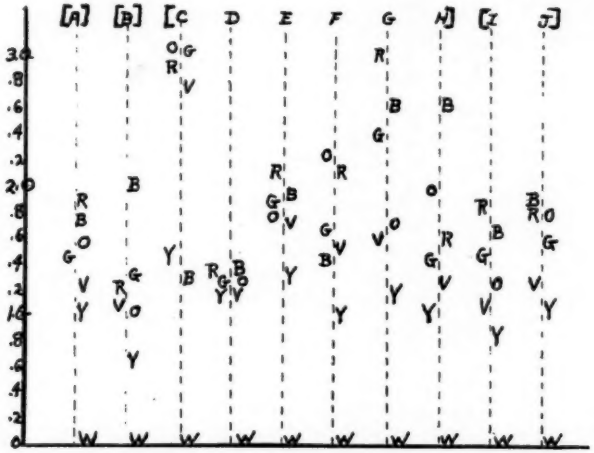

FIG. 1. GRAPHIC REPRESENTATION OF THE SCALES OF THE VARIOUS GROUPS

is seen to be 69 units. It might be permissible to say that all colors had a stronger pull for the first group than for the second group. The ratio for yellow is 105 to 69, or 1.52:1.00. In the first group, red has a scale value of 185, and for the second group a value of 122. Here the ratio (185:122) is 1.50:1.00, or about the same as it was for yellow.

In the case of Group A, R is the most preferred color, being 11 points above B, while, in the case of Group B, R stands third from the most preferred, being transcended by B, 70 units, and by G, 4 units. In the younger group O and V taken as complementary, and R and its complement G have respective ratios of (155:167) 1.20:1.00 and (185:151) 1.22:1.00. In the older group these complementary colors stand each to each so closely that it may be said that the judges experience practically no discrimination of feeling (122:126) between the members of the respective pairs. The writers offer no explanation for this disposition of the difference between B and Y to increase and for that of the others, as R, G, O, and V to decrease.

Analysis of scale for young children. Two questions arise: (1) What are the scales for children for various ages? (2) What differences are found due to sex? Table I gives the scales and Fig. 1 their graphic representation. Scale C is for children

of 1.5 to 2.9 yr., pre-school children. Since this group is small the results are not quite reliable; Scale D is for pre-school children from 3 to 3.9 yr.; Scale E is for pre-school children from 4 to 4.9 yr.; Scale F is for kindergarten children from 5 to 5.9 yr.; Scale G is for kindergarten children from 6 to 6.9 yr.; Scale H is for first-grade children of 6 to 6.9 yr.

On examination of the scales and the graphic presentations of the same, Fig. I, it will be observed that all these children have preferences as indicated between the achromatic white and any color; *i.e.* all place white last, and that they place yellow next to white (with the exception of Groups C and D, the youngest ones, who, however, prefer it definitely to white.) If we ignore Scale C we may say that with increase of age (Scales D to G) these children experience a more definite feeling of preference for the several colors. (Compare the coefficients of variability.) Red is always high in the scales, and is somewhat preferred to blue in most instances. As to Scale H, we would suggest the influence of adult bias.

Sex differences. Among young children Scales I and J are given for 495 boys and 537 girls. Here we find in the case of the boys a tendency for red to have the greatest affective value, and in the case of girls a tendency for blue to have the greatest affective value, with red running a close second, so close that there is hardly a perceptible difference. The scale of the boys is more clean-cut than that for the girls. We cannot say, however, that there are indicated satisfactory sex differences excepting for absence of confusion in discrimination on the part of the boys. Garth[9] found the same situation when he compared older boys and girls, that boys feel more clean-cut differences between the affective values of the several colors.

Summary

(*1*) The older children (1000) have a more definite discrimination of feeling of difference between the colors than the younger children (1032).

(*2*) From 3 to 6 yr. of age in the Kindergarten the coefficient of variability increases with each age group, though there is a slight set-back with 6-yr. olds in the first grade. The discrimination of feeling for colors tends to increase with age.

(*3*) The boys on the whole discriminate the feeling values of colors somewhat better than the girls, even among children of 6 yr. and less.

(*4*) Even children of 1 to 2.9 yr. like any color better than white. This is shown by the incomplete scale, Scale C. The Scale (D) for children 3 to 3.9 yr. is more representative for the youngest group and indicates the same tendency, but no one color has any decided preference with these children.

(*5*) If we ignore the results for the youngest group because of the inadequate number tested, we may say that yellow tends to be the least esteemed of the colors. Red is held in high esteem by all age-groups for children through the Kindergarten. It drops in the first grade, and this suggests adult influence.

(*6*) On the whole blue, with the exception of the 5 to 5.9 yr. group, tends to be more esteemed with increasing age, and the divergence between it and yellow tends to increase with age, with the exception mentioned.

(*7*) There is a tendency for the ratios between the "pulls" of red and green, and orange and violet, to decrease with age.

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[9] T. R. Garth, *Race Psychology*, 1931, 115-136.

THE PITCH OF TONAL MASSES

By ADOLPH G. EKDAHL, University of New Hampshire,
and EDWIN G. BORING, Harvard University

Twenty years ago Baley reported that a number of tones, sounding together and differing from each other by small equal increments, tend to fuse (zusammenfliessen) into a single resultant tone, the pitch of which corresponds to the arithmetic mean of the frequencies of the stimuli.[1] Baley presented data for only four cases: (1) 6 tones in the range 610-625 cycles, (2) 6 tones in the range 600-620 cycles, (3) 8 tones in the range 600-628 cycles, and (4) 10 tones in the range 600-636. In the first case the increment between successive tones of the series was about 3 cycles, in the other cases it was about 4 cycles. The tone resulting from the mixture was described in the four cases as lying respectively in the ranges 616-619, 608-612, 612-618, and 616-620. It is obvious that this work is not quantitatively exact. There is no evidence of a psychophysical procedure and the subjective equivalents were not exactly determined. Baley's use of the phrase "arithmetic mean" does not signify that the equivalence is not for the geometric mean. The arithmetic mean of the six tones in his first case is 617.60 cycles, the geometric mean is 617.57 cycles, and he fixed the subjective equivalent merely as lying between 616 and 619 cycles. Baley, however, stressed more than the quantitative results the intimate fusion of the tones into a single resultant tone.

In 1925 the junior author, working in the Harvard Psychological Laboratory, undertook to determine the subjective equivalents in pitch of the tonal masses described in Table I. The points of subjective equality were computed by the method of constant stimuli with five comparison stimuli and (except for two cases, see legend to Table II) 100 series. The six sets of notes defined in Table I were given by an Appunn reed tonometer and were employed as the standard stimuli. The standard was given first. The stops were pulled out, the air was turned on and the tonal mass allowed to build up, then the stops were all pushed in at once by a hinged felt-covered board, and the single stop for the comparison stimulus was pulled out. The *O* made comparative judgments of the pitch of the single note in respect of the preceding tonal mass.

There was no evidence at all of the fusion of any of these tonal masses to yield a single resultant tone or note. The mixtures always sounded complex, and the psychophysical judgment was a judgment of equivalence of pitch but by no means a judgment of identity. This resultant is not a direct contradiction of Baley's finding. The minimal increment in the present experiment was 8 cycles in the region of 264 cycles, where 8 cycles is equivalent to about 52 musical cents (100 cents = 1 semi-tone in equal temperament). Baley used steps of about 4 cycles, but they were in the region of 620 cycles where every step is only about

* Accepted for publication March 1, 1933.

[1] S. Baley, Über den Zusammenklang einer grösseren Zahl wenig verschiedener Töne, *Zsch. f. Psychol.*, 67, 1913, 261-276.

11 cents. However, the minimal range in the present experiment ("4 by 8," Table I) was 157 cents, and for it there was no good fusion; whereas the maximal range for fusion reported by Baley—case (4) cited above—was 102 cents, a quarter-tone less. Nevertheless it seems doubtful that Baley's tones could have fused in the way that colors fuse in color mixture to give identity in perception. It is much more likely that his "resultant tone" was merely a tone judged as equivalent to a perceived tonal mass.

It was, however, not the primary purpose of the present experiment to test Baley's results. Rather we sought to examine the nature of the pitch of the noises which are generated as tonal masses. The complexes were chosen to yield results for different numbers of notes combined (from two to eight) and different ranges of pitch (from about three-fourths of a whole tone to almost an octave). Thus

TABLE I

DESCRIPTION OF STANDARD STIMULI

The tonal masses defined in the table served as standards to which single notes were equated in pitch by the method of constant stimuli. The range is given in musical cents (1200 cents = 1 octave)

Name	No. notes	Int. betw. notes	Notes in tonal mass (cycles)	Geom. mean	Arith. mean	Range in musical cents
8 by 8	8	8	236–244–252–260–268–276–284–292	263.36	264	362
4 by 8	4	8	252–260–268–276	263.85	264	157
8 by 24	8	24	180–204–228–252–276–300–324–348	258.10	264	1141
4 by 24	4	24	228–252–276–300	262.63	264	475
2 by 24	2	24	252–276	263.72	264	147
2 by 48	2	48	240–288	262.91	264	315

in the stimulus '2 by 24' we have an ordinary interval that is something less than a major second, and in the stimulus '2 by 48' we have an interval a little more than a minor third. These pairs are simple fusions. However, the stimulus '8 by 8,' extending over an interval slightly less than a major third, yields a confused mass that can readily be described as a noise, and the stimulus '8 by 24,' covering nearly an octave, is also a noisy complex.

The results are shown in Table II.[2] Here the small size and variability of the intervals of uncertainty show that the judgment was difficult and that the *Os* tended to avoid the category *equal*, as they might not have done had it been possible for *equal* to mean *identical* instead of merely *equivalent*. Hence we may dismiss the intervals of uncertainty as unimportant.

The first line of the table shows that *Ds* with the '8 by 8' stimulus gave the point of subjective equality at 261.46 cycles. This value is quite close to the mean of the stimulus (range 236–292 cycles) which is 264 cycles. It will be observed from the values of *h* that the point of subjective equality, 261.46, is quite precisely established. The interquartile range in this case is about 6 cycles, *i.e.* with a change of 6 cycles one passes from the stimulus that yields 25% judgments of *higher* to a stimulus that yields 75% of *higher* (or conversely for *lower*).

Examination of Table II shows that, with the exception of the case of '8 by 8'

[2] The observers in this experiment were H. R. DeSilva, A. M. Brues, R. A. McFarland, K. E. Zener, and T. F. Karwoski, then research students in the Harvard Psychological Laboratory.

TABLE II

SUBJECTIVE EQUALITY OF SINGLE NOTES TO TONAL MASSES

The first two columns show the number of notes and the interval (cycles) between successive notes in the tonal masses making up the standard (cf. Table I). The body of the table gives, in cycles, the lower and upper limens, the values of h for each of the limens, the interval of uncertainty, and the point of subjective equality (intersection of the two psychometric functions). A value of $h = 0.2$ corresponds to an interquartile range on the psychometric function of about 4.8 cycles, since P.E. $= 0.4769\ h$. All frequencies in the psychometric functions are based on 100 observations, except that there were only 75 observations for *Ze* and 85 for *Ka*.

	Standard		Limens		h		Interval of Uncertainty	Point of Subjective Equality
O	No. notes	Int. betw. notes	Lower	Upper	Lower	Upper		
Ds	8	8	260.30	262.51	.1502	.1648	2.21	261.46
Br	8	8	254.57	255.19	.2331	.2426	0.62	254.89
Mc	8	8	251.63	251.95	.1845	.1893	0.32	251.79
Ze	8	8	256.67	260.16	.1185	.1612	3.49	258.68
Ka	8	8	284.81	276.37	.0720	.0895	−8.44	280.13
Ds	4	8	261.75	264.70	.2492	.2051	2.95	263.08
Br	4	8	259.64	259.72	.4514	.4426	0.08	259.68
Mc	4	8	260.47	263.46	.2061	.2218	2.99	262.02
Ds	8	24	262.91	265.11	.2384	.2222	2.20	263.97
Br	8	24	271.47	271.45	.1398	.2656	−0.02	271.46
Mc	8	24	285.38	285.38	.1971	.1971	0	285.38
Ds	4	24	266.07	267.75	.1983	.1534	1.68	266.80
Br	4	24	252.16	254.27	.0940	.2509	2.11	258.69
Ds	2	24	266.21	269.12	.2077	.1307	2.91	267.33
Ds	2	48	238.45	242.24	.2238	.1643	3.79	240.05
Br	2	48	240.04	241.90	.4121	.3649	1.86	240.91

for *Ka* and the two cases of '2 by 48,' all the points of subjective equality are near the mean, 264, and that nearly all of them are placed within interquartile ranges of less than 6 cycles. This degree of precision is high, although it is not great enough to give significance to the question as to whether it is the arithmetic or the geometric mean that is approximated (cf. Table I). What the Table II shows is that an accurate judgment of pitch can be made for tonal masses where as many as eight tones, distributed within a musical third or over an entire octave, are sounded together. In such cases the pitch of the mass is taken by *O* as equivalent to a mid-pitch of the mass. The same principle held in this experiment for masses of four tones, and for a two-tone combination when the two tones were separated by an interval less than a major second ('2 by 24'). It did not hold for an interval approximating a minor third ('2 by 48').

There were three exceptions to the general rule. In the case of '2 by 48' both *Ds* and *Br* equated the pitch of the pair to the pitch of the lower member of the pair. We probably have here the limiting case where the number of components is so few (two) and the separation is so great (minor third) that the *O*, in making a judgment of pitch, could not avoid discriminating the two pitches

and selecting one as the equivalent. It is apparent that a complex tonal mass or one within a small range of pitches is localized as to pitch at its mid-pitch, but that simpler complexes may be characterized by one of the actual pitches in the complex. Thus we know that clangs are located as to pitch by their fundamentals, the lowest pitch of the complex, but that in musical harmony the upper note of a chord often characterizes the fusion as to pitch.

The other exception is the case of '8 by 8' with *Ka*. He found the judgment very difficult and his subjective assurance was low. His psychometric functions show erratic frequencies, as the negative interval of uncertainty and the low values of *b* indicate. The point of subjective equality (280.13) is well above the average, but not as high as upper note of the stimulus (292). Presumably this case can be ignored, as dependent upon an instable attitude.

In brief we may say that, if we start with a single tone and add successively to it other tones that bear no particular musical relation to one another, we pass through musical fusions to confused tonal masses which are noisy and which may be regarded as noises approximately located as to pitch. The nature of these complexes as noises is most obvious when the masses are given briefly and explosively by an abrupt quick pressure on the tonometer bellows. The fact that they have location in the scale of pitches is shown by the capacity of *O*s to equate them to simple pitches, usually a pitch approximating the pitch that corresponds to the mean of the frequencies of the tones in the mass. However, the mass is still perceived as a mass, and its equation to a single pitch is only a rough localization that resembles the manner in which a large cutaneous impression is localized, when its exact position is required, approximately at its mid-point.

In this manner we come to see the relation of noises to tone. Complexes of tones are noises, and a noise is tonal when pitches within it are continued and when the range of pitches allows of some relatively definite location of the complex noise in the scale of pitches.

CERTAIN QUANTITATIVE ASPECTS OF CHICKENS' BEHAVIOR TOWARDS FOOD

By W. A. Bousfield, Harvard University

In an earlier paper, the writer has attempted to show that graphical records of the amount of food eaten by cats during a period of feeding possess a high degree of regularity.[1] The analysis of these records demonstrated that they could be described adequately by means of the exponential equation,

$$f = c\,(1 - e^{-mt}) \quad \ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots [1]$$

in which f = amount of food eaten, c = constant, e = base e of natural logarithms, m = constant, and t = time.[2] The experiment reported here was undertaken with the purpose of applying a parallel quantitative method to a study of the behavior changes of a different animal during the eating of a different food.[3] These data not only furnish an index of the variability of a certain type of behavior, but also a partial check on the generality of the exponential equation as a descriptive formula.

TABLE I

Values of Constants c and m

	Chicken A		*Chicken B*		*Chicken C*		*Chicken D*	
Record	c	m	c	m	c	m	c	m
1	180	.0329	180	.0366	180	.0478	180	.0413
2	180	.0468	180	.0410	200	.0470	180	.0426
3	180	.0512	180	.0563	200	.0476	180	.0408
4	180	.0512	180	.0413	180	.0444	180	.0393
	Average $c = 182.50$				Average $m = .0442$			

An experimental technique was devised to obtain kymograph records of the amounts of whole-wheat eaten by chickens during regular periods of feeding. Four Rhode Island Red roosters were used for *Ss*. At a regular hour each day an *S* was admitted to a pen containing a trough 2 in. deep and 18 in. square, from which the animal pecked its food as it was dropped through a piece of brass tubing extending at an angle of 30° from the side of the pen to the edge of the trough. Portions of 2 grm. were presented at a time. As soon as all but about 6 kernels of the wheat had been eaten, *E* dropped another 2-grm. portion into the tube, and at the same time depressed a telegraph key which was wired to a recording de-

* Accepted for publication January 20, 1933.

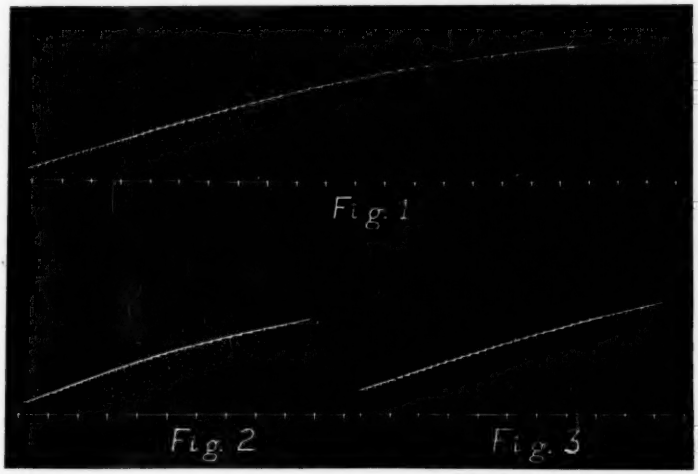
[1] W. A. Bousfield, Certain quantitative aspects of the food-behavior of cats, *J. Gen. Psychol.*, 8, 1933, 446-454.

[2] The exponent, mt, was originally presented as positive. The negative exponent provides a simpler mathematical significance.

[3] The writer wishes to thank Dr. M. H. Elliott of the Harvard Psychological Laboratory for his help in the undertaking of this experiment, and Professor G. D. Birkhoff of Harvard Mathematics Department for his assistance in the interpretation of the exponential equation.

vice arranged to lift a writing-point vertically through a unit-distance. The process was continued until the animal definitely ceased to respond to the food, *i.e.* sat down and closed its eyes, began preening its feathers, or walked to the opposite side of the pen. A screen concealed all movements of *E*. The recording apparatus which was somewhat noisy in its operation was placed in an adjoining semi-sound-proof room.

In all, four records were obtained from each chicken. Typical samples are presented in Figs. 1, 2, and 3. The method of treatment of the records was similar to that employed in the earlier experiment with the cats. Again, the exponential equation [1] was found to give curves closely approximating the original records. The values of constants c and m of all the records are presented in Table I. These



FIGS. 1, 2, AND 3. ORIGINAL RECORDS WITH THEORETICAL CURVES SUPERIMPOSED
Each vertical step on the records represents the consumption of two grm. of whole-wheat. Time has been recorded horizontally in minutes.

constants have been calculated so that t represents time in mins. and the f represents grm. of food eaten. It should be noted that the mathematical treatment consists in first selecting a c to give the best alignment of the plotted points, and then in calculating m on the basis of the slope of a straight line drawn through the plotted points.[4] In Figs. 1, 2, and 3, the theoretical curves have been superimposed upon the original records.

The examination of Equation [1] shows that it is really a descriptive formula. The constant c is the horizontal asymptote of the function, and thus represents the

[4] The accuracy of c is limited to $\pm$ 5 units, with the longer records furnishing more accurate determinations. The method of treatment may slightly exaggerate both the constancy of c and the variability of m.

quantity of food which is approached as a limit by the function and theoretically never completely realized. If we call c the ideal physiological limit of the amount of food consumable by the animal, we shall not be so much interpreting c as stating what the formula shows c actually to be. It may be that c will turn out to depend upon the capacity of the animal, for in the data of this experiment c shows little individual difference and the four roosters, all belonging to the same brood, were similar in physical development. On the other hand, m varies with the rapidity with which the function approaches its asymptote; the more rapidly the animal consumes food the larger is m. Accordingly, m may be described by calling it the *coefficient of voracity* (for m is a coefficient in some expressions).[5] Thus m shows larger individual differences than c and less consistency for the individual, as might be expected for a characteristic which depends on a higher order of reflex integration. Still further assumptions, however, are justified. The rate of eating at any time may be determined by differentiating Equation [1],

$$\frac{df}{dt} = cme^{-mt} \quad [2]$$

But from Equation [1], it may be seen that

$$ce^{-mt} = (c - f) \quad [3]$$

Equation [2], accordingly, can be rewritten,

$$\frac{df}{dt} = m(c - f) \quad [4]$$

In other words, the rate of eating, at any time, varies directly with the difference between what the animal has already eaten, f, and its ideal physiological limit, c.

In spite of the differences in the rates of eating, the theoretical curves are approximated regardless of the durations of the times represented by the graphical records. A shorter record may, for this reason, be considered as a segment of a longer record which might have been obtained under more ideal conditions. With respect to the theoretical rate, the behavior tends to be all-or-none. Cessation of eating thus implies a definite shift in the behavior-pattern. It is not surprising to find that this change in behavior takes place progressively more readily as the animal continues to eat, since distension of the digestive apparatus must be paralleled by some sort of reflex inhibition. In proportion as the amount of this inhibition increases, the effectiveness of competing stimuli is enhanced.

It is evident from the foregoing analysis that the exponential equation is fundamentally reasonable as a descriptive formula when applied to certain quantitative aspects of an animal's behavior towards food. The constants of the equation are significant in their implications since they may be related to the meaningful concepts of an ideal physiological limit of food-consumption and a coefficient of voracity. The determination of the numerous factors influencing these constants offers an attractive field for further research.

[5] The concept of a coefficient of voracity was suggested to the writer by Dr. G. D. Birkhoff. The term possesses the advantage of psychological significance while at the same time the relation to the mathematical description is immediately implied.

A COMPARISON OF SIMULTANEOUS AND SUCCESSIVE METHODS OF MOTOR LEARNING

By LELAND W. CRAFTS and R. MORRIS ALLEN, New York University

The object of the experiment was to determine whether an activity requiring ultimately the simultaneous use of the two hands—in this case to trace two identical designs seen in a mirror—would be better learned by practicing it first with each hand separately (the 'part' or 'successive' method), or by practicing with both hands together from the beginning (the 'whole' or 'simultaneous' method).

The only previous experiment of this type was performed by Koch,[1] who had her *Ss* learn to make with each hand simultaneously nine finger reactions on five keys of each of two typewriters in a fixed sequence and tempo. Her results show the successive group to have been reliably superior to the simultaneous. The present investigation was undertaken with the aim of finding out whether similar results would be obtained with an act different from that which she employed.

METHOD AND PROCEDURE

Subjects. The *Ss* were 40 undergraduate students in psychology, assigned alternately as they presented themselves for the experiment to the successive and the simultaneous groups. The former contained 15 men and 5 women, the latter 17 men and 3 women.

Apparatus. The apparatus consisted of a mirror-drawing set-up as follows. A mirror 25.5 x 19.5 in. was held by clamps and wire in a vertical position on a table. The design to be traced (a four-pointed star having eight 'alleys' each 2¼ in. long and 5/16 in. wide) was fastened to a drawing board in front of it. A curtain of black cloth prevented *S* from seeing the designs in any way save in the mirror. Two electric lights, one on each side of the mirror, provided the necessary uniform illumination. All the *Ss* used hard lead pencils furnished by *E*.

Procedure. The successive group practiced with each hand separately, using the preferred hand first (which was by chance always the right), until they could trace the design with each hand within 1 min. and with not more than one error. They then practiced tracing two stars with both hands simultaneously until they could do so in 2 min., and with not more than two errors with each hand. For this work the two designs were placed as close together as possible. The *Ss* were told that the movement of the hands must be coördinated. While a literally perfect simultaneity was not obtained, no *S* was allowed to have his hands farther apart in the course of the tracings than a distance of one half an alley on the star. If *S* failed to maintain this degree of coördination he was warned by *E* and any *S* for whom three such warnings were required was eliminated from the experiment. Altogether the records of 6 *Ss* were for this reason discarded.

The simultaneous group practiced with both hands simultaneously from the be-

[1] H. L. Koch, A neglected phase of the part-whole problem, *J. Exper. Psychol.*, 6, 1923, 366-376.

ginning until they achieved the criterion stated above. Coördination of the two hands was insisted upon to a similar degree throughout.

All *S*s were instructed to stress accuracy rather than speed. They were also told, when they made an error, to return as nearly as possible to the place of departure from the alley without lifting their pencil from the paper. The practice may be termed massed, since the interval between trials was only about 0.5 min. Scoring was in trials, errors and time. An error was defined as actually crossing with the pencil either boundary line of the star. Touching such a line was not scored as an error, nor was the recrossing which necessarily occurred when *S*, having made an error, had to return to a point within the design.

TABLE I

GIVING THE AVERAGE SCORES FOR BOTH GROUPS AND ALL CRITERIA

	Right hand			Left hand			Both hands together		
	Trs.	Errs.	Time	Trs.	Errs.	Time	Trs.	Errs.	Time
Simultaneous		61.35			58.40		7.35	119.75	1157.60
Successive:									
Each hand separately	1.68*	9.20	252.80	1.25	3.95	105.40			
Both hands together							3.15	21.95	448.65
Total, hands separately and together							6.08	35.10	806.85
D./S.D.$_{diff.}$							1.71	4.59	3.04

* In computing the above averages a trial with one hand separately was counted as 1/2 a trial only. Hence the average number of actual tracings with the right hand alone was 3.36 and with the left hand 2.50.

RESULTS

The results, given in Table I, show that the successive group of *S*s was superior to the simultaneous group in performance. The differences are marked according to all three criteria, and are reliable for errors and for time. To find the successive method superior to the simultaneous for learning an activity of this type confirms the conclusions of Koch before mentioned.[2] Her group differences were least for errors, ours are least for trials. Otherwise the results of the two experiments are in complete agreement.

In explanation of this superiority of the successive method the following factors are probably significant.

(*1*) There was presumably in the successive group a considerable transfer from the initial practice with the preferred hand to the later practice with the non-preferred. While our data do not prove this point—a preliminary test with the left hand was not made—Ewert has shown that marked bilateral transfer does occur in mirror-drawing.[3] Furthermore, as McGeoch's review of the experimental literature on the whole-part problem shows,[4] it is generally agreed that, in maze learning at least, the possibility of positive transfer between one part and another

[2] Koch, *op. cit.*
[3] P. H. Ewert, Bilateral transfer in mirror drawing, *Ped. Sem.*, 33, 1926, 235-249.
[4] G. O. McGeoch, Whole-part problem, *Psychol. Bull.*, 28, 1931, 713-739.

constitutes a condition favoring a part method. It is very probable that the successive method for the learning of two separate acts which must later be integrated would also be favored thereby, and that this is therefore one cause for its superiority to the simultaneous method in the present experiment.

(*2*) Koch notes that in her experiment the difference in favor of the part method was less with those *Ss* who had had the most experience in playing the piano. Since these were also the *Ss* for whom her task was the easiest, she concludes: "This suggests that the advantage of the part method over the whole is greatest when the task is most unfamiliar and difficult."[5] Pechstein holds similar views.[6] While this factor of difficulty can scarcely be expected of itself to determine the relative values of part and whole methods to the exclusion of all other conditions—that it does not always do so has been demonstrated by Crafts with card-sorting[7]—it may plausibly be regarded as having some significance. In the present experiment mirror-drawing was for all *Ss* a totally unfamiliar task and to trace the designs with both hands simultaneously seemed to be difficult and, if not preceded by practice with each hand separately, very confusing as well. Hence in so far as this factor is important it may perhaps have favored the successive method.

[5] Koch, *op. cit.*, 376.

[6] L. A. Pechstein, Massed vs. distributed effort in learning, *J. Educ. Psychol.*, 1921, 12, 92-97.

[7] L. W. Crafts, Whole and part methods with non-serial reactions, this JOURNAL, 41, 1929, 543-563.

WORK-SCORES *VS.* TIME-SCORES

By QUINN MCNEMAR, Stanford University

In order to decide upon the relative merits of work-scores as compared with time-scores, it seems necessary to give consideration to three phases of the problem. In the first place, it would be better to use that score which gives distributions more nearly normal, thereby permitting the use of ordinary statistical techniques. It often happens that time-scores give skewed distribution curves, whereas the same performance scored in terms of work per unit of time will result in distributions approximately normal. In the second place, the question of the relative speed of work is of theoretical importance as regards the effect of two different mental sets upon performance. This may manifest itself in group trends or may result in wide individual differences. In the third place, the accuracy with which one score can be predicted from the other is of fundamental importance in the theory of measurement and in the practical situation wherein variables are being intercorrelated. Although it would seem safe to assume that the two types of scores would correlate with each other as highly as their reliabilities would permit, such an assumption should be checked in order to satisfy the skeptical investigator.

The study of Paterson and Tinker demonstrates that for a speed of reading test,[1] the correlation of work-limit with time-limit scores was unity when corrected for attenuation. That such a finding may not hold for types of performances requiring skill was brought to the present writer's attention in connection with a study of the effect of practice upon twin resemblances in motor skills. In this study, the assumption was made that the reciprocals of the work-scores would be proportional to the corresponding time-scores, but when twin resemblances were computed on the basis of these reciprocal scores, the coefficients of resemblance were found to vary considerably from those obtained when the work-scores were used. It is the purpose of this paper to present the results of an experiment so designed as to permit an empirical check on the interchangeability of time- and work-scores for two of the performances used in the twin study.

Fifty men (elementary psychology students) were given 12 trials on each of two tasks, spool packing and card sorting, the odd trials being scored in terms of work done in a given time (60 sec. for the spools and 30 sec. for the cards) and the even trials being scored as the time to do a given amount (72 spools and 30 cards). The performance of a 'control' group of 50 men was obtained for each task, both odd and even trials being scored in terms of work done in 60 sec. for the spool packing and 30 sec. for the card sorting. The order of trials for both groups was: 6 trials on the spool packing, 6 on card sorting, 6 more on the spool packing, and 6 more on the card sorting. With the exception of 8 *Ss* who worked singly, all the *Ss* were run two at a time, two spool packers and two card sorting trays being used. The social factor thus introduced seemed to

[1] D. G. Paterson and M. A. Tinker, Time-limit vs. work-limit methods, this JOURNAL, 42, 1930, 101-104.

aid in sustaining the interest of the *S*s and probably served as a motivating factor. The *S*s of the experimental group were told that the purpose of the experiment was to compare the rate of work under the two conditions, while the 'control' *S*s were told that their services were needed in order to establish comparative norms for the two tasks. The time required of each *S* was approximately 35 min.

It should be noted that the odd and even trials of the 'control' or work *vs.* work-group were given under exactly the same conditions, whereas for the work *vs.* time trials of the experimental group a variable factor was present; namely, the unknown effect on the performance of the slower individual of a pair of the timed trials, which resulted from a knowledge that his partner had finished ahead of him. This factor, if operative, would probably tend to reduce the predictability of work from time-scores. Otherwise, the chief difference between the two groups concerns the mental set involved in the time-limit as compared with the work-limit trial.

TABLE I

PREDICTION OF WORK- FROM TIME-SCORES

(N = 50 for each group)

	Group	Type	r	S.D.$_{w}$	Standard error of estimate and its σ	r adjusted for range
Spool packing	I	W *vs.* T	.888	5.08	2.34 ± .23	.925
	II	W *vs.* W	.959	7.15	2.03 ± .20	.945
Card sorting	I	W *vs.* T	.951	3.30	1.02 ± .10	.954
	II	W *vs.* W	.937	3.49	1.22 ± .12	.934

TABLE II

COMPARISON OF RATE OF WORK UNDER THE TWO CONDITIONS

	Group	Type	Means and their σ		$D_M \pm \sigma_D$
Spool packing	I	W *vs.* T	68.7 ± 0.7	68.9 ± 0.7	.2 ± .3
	II	W *vs.* W	69.3 ± 1.0	70.1 ± 1.0	.8 ± .3
Card sorting	I	W *vs.* T	29.9 ± .5	29.9 ± .5	0.0 ± .2
	II	W *vs.* W	29.6 ± .5	30.3 ± .5	.7 ± .2

The data permit the determination of the relative accuracy of prediction of work scores from time-scores and work-scores from work-scores. The results of this analysis are given in Table I, from which it will be seen that there is no significant difference in the standard errors of estimate for work-scores predicted from time-scores (Group I) and work-scores predicted from work-scores (Group II). Or considering the correlation coefficients as adjusted for range (for 100 *S*s, S.D. for spools is 6.21, for cards, 3.40, each based on odd trials, *i.e.* work-scores), it will be noticed that there is little difference between the two coefficients for the two groups. Stated in another way, the relationship between work- and time-scores in each case (spool packing and card sorting) is as high as the self-correlation will permit. This is in agreement with the results of Paterson and Tinker for a more intellectual function and based on group rather than individual testing.

In order to check the rate of work under the two conditions, the time-scores

were converted to rate per 60 sec. for the spool packing and per 30 sec. for the card sorting. Table II gives the means (in comparable rate terms) for the odd and even trials for each group. Group II shows a significantly greater average rate on the even trials for both tasks, the difference being due probably to the practice advantage in the even trials. The absence of this difference in the case in which the even trials were timed (Group I) may be due to a slower rate under this condition. It should be noted, however, that this slower rate cannot be said to possess statistical significance, and that further experimentation is needed in order either to prove or disprove the existence of a difference in the rate of work under these two conditions.

The results of this study tend to support the view that work-limit scores are interchangeable with time-limit scores, and it is concluded that, except in the case where skewed distributions result from one type of score, it should make little difference in the practical situation which type of score is used. This does not, of course, apply to the highly paradoxical situation met with in connection with the study of the effect of practice on group variability.

MINOR STUDIES FROM THE PSYCHOLOGICAL LABORATORY OF THE UNIVERSITY OF NEBRASKA

VIII. The Dependence of Hue, Tint and Chroma upon Area

By Beatrice H. Marshall and J. P. Guilford

In recent months the senior author has had several occasions for using the *Munsell Book of Color* (Abridged ed.) in the evaluation of colored papers. Those who are familiar with the book know about the arrangement of colors included within its pages. Each one of twenty pages contains small rectangular samples of color all of the same hue but varying in tint (value) and in chroma. The vertical dimension of the page represents differences in tint, with the darkest colors at the bottom and the lightest at the top. There are usually eight distinct steps in tint, with psychologically equal differences between them. The horizontal dimension of the page represents differences in chroma. The column at the left has the least saturation and the chroma increases by equal steps as one goes to the right side of the page.

The *Book of Color* is provided with a gray mask with a rectangular opening equal in size and shape to the colored rectangular samples on its pages. This is for the obvious purpose of equating the area of colored material which is to be evaluated with the area of the standards in the book. Some of the senior author's *Os* were of the opinion that a colored sample, 4 in. sq. for example, would rate the same with or without the mask. Others were of a more decided opinion that without the mask there would be a distinct change in the rating; that the color square would rate much higher for chroma when viewed without the mask.

From a practical standpoint if it could be shown that the size of the color to be rated makes no significant difference in the evaluation of it on the Munsell scheme, then the use of the mask, in so far as it is used to control size, could be dispensed with and time could be saved. If, on the other hand, size is an important factor, then the mask must always be employed. From a theoretical standpoint, also, it would be interesting to know whether there is any change in hue, tint, or chroma as the size of the stimulus is changed. Do larger areas increase in lightness and darkness? Do larger areas appear to be more saturated than smaller ones? Does the hue of a color itself tend to shift in any systematic manner as the size of the stimulus is changed?

One finds very little in the literature that bears directly upon this point, although the dependence of chroma upon field size seems to have general recognition. To mention only a few, Abney,[1] in speaking of the extinction of different colors with low intensities of stimulation says, "The intensity of the light to just cause a loss of colour may be increased tenfold when the aperture is diminished to

* Accepted for publication July 12, 1933.

[1] Wm. Abney, *Researches in Colour Vision and the Trichromatic Theory*, 1913, 156.

one-eighth the diameter." Purdy[2] agrees with Abney "that smaller field sizes gave higher values for both the specific (chromatic) and general (light) threshold." Judd,[3] in working upon sensitivity to differences in color temperature, found that the ability to discriminate differences depended upon field size. It is generally known that in peripheral vision the larger the color patch, the farther in the periphery it may be seen as color. The sizes of the traditional color zones increase with larger stimuli.

Certain facts connected with color-blindness also lead one to conclude that the greater the field size the greater the saturation. Color-blind individuals are commonly known to respond to red or to green or to both as colors when the stimuli are made large enough or saturated enough. Color-blindness is probably merely extreme color weakness. Nagel, for example, a typical deuteranope, could readily distinguish red if the field size was sufficiently great.[4] Tests of color-blindness take account of this fact by making the stimuli in the form of small spots of rather low saturation, as in the Nagel, Stilling, and Ishihara tests.

All these observations have to do with very low intensities of stimuli, as in the work on chromatic thresholds and the colorless interval, or else with the abnormal eye that has a very high threshold for some of the hues. The question that we are raising has to do with higher degrees of saturation and with normal eyes. Undoubtedly, under the special conditions just mentioned, the size of the stimulus is an important factor for chroma. It is conceivable that with medium intensities of illumination, normal eyes, and with sizes of stimuli such as were used in this study, the limits of the effect of size have been reached. Casual observations previously made during the use of the *Munsell Book of Color* seemed to show that those limits had not been reached. The present study deals just with that phase of the problem. A much more thorough approach would make use of an extensive range of stimulus intensities and of field sizes. The quantitative relationships between chroma and field size would then be established from the lower limit, the chromatic threshold, to the upper limit, the size beyond which no further increase in saturation is possible for a given degree of illumination and for the normal eye.

Stimulus materials. Fifteen different colors were chosen from the Milton Bradley series of 90. First, 5 representative hues were selected. The most saturated sample of each hue, and one lighter and one darker sample of it, were chosen. The entire range of values was from about 3 to 9 on the Munsell scale. The range in chromas was from about 6 to 14. The names of the colors will be found in the first column of Table I. In addition to these 15 colors, samples of black and white were used. These 17 samples were prepared in the form of squares in 4 sizes. The areas were 0.25, 1, 4, and 16 sq. in., so planned as to represent a geometric series. The squares were pasted on a gray background 8.5 x 8.5 in., the background having a rating of N 5 on the Munsell scale.

Procedure. The observations were made in a northeast room with ample indirect

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[2] D. McL. Purdy, Saturation and chromatic thresholds of spectral colours, *Brit. J. Psychol.*, 21, 1931, 296.

[3] D. B. Judd, Precision of color temperature measurements, *Bur. Standards J. of Res.*, 5, 1930, RP No. 252, 1169.

[4] W. Nagel, Farbenumstimmung beim Dichromaten, *Zsch. f. Sinnesphysiol.*, 44, 1910, 5-17.

sunlight illumination, between the hours of 10 A.M. and 3 P.M. *O* sat at a headrest and the color sample was placed directly in front of him against a larger gray background 22 x 28 in. in size, and rating N 5 on the Munsell scale. The distance was constant at about 30 in. *O* first chose the page from the Munsell Book that was nearest to the sample in hue, and gave to the sample a rating for hue. He interpolated between pages when no single page exactly resembled the sample. The pages of the book had been numbered, beginning with R which was given a value of 1. After evaluating the sample for hue, *O* then gave his estimate of its tint and its chroma, interpolating whenever necessary. In all these comparisons the pages of the book were set up beside the sample on the large gray background.

The order of presentation of the samples was a prearranged one in which sevral rules were followed. No hue was followed by the same hue in a different size, tint, or shade. No hue followed its complement or near complement. No square was followed by a square of the same size. The order was completely reversed for the different trials. *O* rated the 68 samples during a single sitting, but at least 2 days intervened between sittings. Nine *O*s, with normal color vision, including 2 men and 7 women, made 10 complete sets of ratings.

Results. Since the ratings given the various samples by different *O*s agree so very closely, we have pooled the 90 judgments for each one of the 68 samples, and we present the means in Table I.

Table I demonstrates a striking uniformity of the ratings for any one color sample regardless of its size. Since this is typical of the results of all the *O*s taken individually, this uniformity in Table I is not therefore the consequence of pooling results and the smoothing out of individual differences.

There are one or two significant facts in Table I. Incidentally, the table shows that colors which bear the same name for hue in the Milton Bradley series may not have exactly the same hue. For example, OR has a rating of about 1.75 for one sample, and a rating near 1.10 for the other two. The 3 samples of OY vary from about 4.10 to 4.35. The samples of G vary from about 7.50 to 8.00. The samples of GB vary from about 14.10 to 15.00. Only the BV has a rather uniform rating for hue in the 3 samples used here. The other 2 samples of BV in the Milton Bradley series might not, of course, do so well. The authors had found previously that the Milton Bradley colors cannot be depended upon for uniformity of hue, nor for an even spacing along the color circle.

The only relation of size to the tint of the stimuli that can be discerned in Table I is in the case of black and white. There is a very slight but continuous tendency for the black to gain in darkness with increase in size, and for the white to gain in lightness. This led us to wonder whether the lighter of the colored samples also tended on the average to rise in tint, and whether the darker of the colors tended to fall in the scale. Taking the 8 colors that rated above 5 in value and the 7 that rated below 5, and averaging the ratings for each size, we found decidedly no such difference. The lighter colors gave means of 7.27, 7.30, 7.27 and 7.22 for the 4 sizes respectively. The darker colors gave means of 4.01, 4.03, 4.03, and 4.22 for the 4 sizes. Only black and white show any tendency at all to change in tint as the area is varied.

It had been expected that chroma would be most affected by variations in area. An examination of the data for single colors in Table I will show that in some

TABLE I

COMBINED RATINGS FOR HUE, TINT, AND CHROMA FOR 68 SAMPLES OF COLOR IN FOUR SIZES AS GIVEN BY 9 Os

	Ratings for hue				Ratings for tint				Ratings for chroma			
Size	0.25	1.00	4.00	16.00	0.25	1.00	4.00	16.00	0.25	1.00	4.00	16.0
ORt2	1.70	1.73	1.85	1.80	7.69	7.72	7.61	7.71	7.20	7.09	7.09	7.13
OR	1.14	1.13	1.15	1.16	4.59	4.50	4.58	4.68	14.09	14.24	14.03	14.09
ORs2	1.08	1.06	1.07	1.08	3.02	3.16	3.23	3.32	9.70	10.14	9.86	10.28
OYt2	4.13	4.14	4.11	4.11	8.61	8.74	8.69	8.67	6.23	6.30	6.34	6.35
OY	4.31	4.38	4.36	4.33	8.04	8.04	7.89	7.97	10.03	10.20	10.33	10.46
OYs2	4.05	4.08	4.06	4.19	5.04	5.16	5.15	5.11	8.31	8.28	8.47	8.49
Gtl	7.99	7.99	8.00	7.98	7.67	7.71	7.69	7.42	7.51	7.66	7.47	7.64
G	7.82	7.78	7.82	7.70	7.02	7.02	6.98	6.98	9.09	9.09	9.33	9.43
Gs2	7.52	7.51	7.53	7.52	4.70	4.72	4.75	4.76	7.58	7.53	7.53	7.69
GBtl	14.09	14.04	14.11	14.17	6.89	6.87	6.93	6.87	6.32	6.28	6.49	6.37
GB	15.05	14.95	14.92	14.89	4.87	4.86	4.93	4.86	10.49	10.73	10.66	10.69
GBs2	14.93	14.93	14.88	14.97	2.92	2.95	2.88	3.98	7.56	7.37	7.64	9.86
BVt2	16.05	16.02	16.05	16.02	7.19	7.11	7.24	6.99	5.91	6.52	6.31	6.92
BV	16.03	15.98	15.98	15.95	4.84	4.76	4.69	4.75	10.32	10.52	10.76	10.63
BVs2	15.94	15.94	15.98	15.94	3.13	3.26	3.18	3.17	9.11	9.19	9.14	9.58
Black					1.64	1.63	1.59	1.56				
White					9.54	9.79	9.76	9.83				

cases a distinct trend does exist. This trend is an increase in chroma with increase in area. But in most cases the trend is uncertain. We have found the means of the ratings for chroma for the 5 lightest, the 5 darkest, and the 5 medium colors representing the 5 hues. For the lightest colors they are: 6.63, 6.77, 6.74, and 6.88 for the 4 sizes from smallest to largest. The total gain in chroma from smallest to largest is only 0.25 unit, certainly not anything very significant. For the medium colors the means are: 10.80, 10.96, 11.02, and 11.06. The gain is nearly the same; namely, 0.26 unit. For the darkest 5 colors the means are: 8.45, 8.50, 8.53, and 9.18. Here there is a more decided gain of 0.73 unit. A very large part of this gain can be attributed to one color, the GBs2. This color, for some unknown reason, took a sudden jump of about 2 units in chroma and at the same time a jump of about 1 unit in tint. The samples of any one color were originally cut from the same piece of colored paper material. This particular sample, the 4 in. sq. of GBs2, was different enough that the *Os* sometimes remarked about it. We are unable to account for this sudden 'sport' among the colors. Even with this sample left out, however, the trends of which we speak were decidedly present. The mean of the chromas with the unusual sample omitted was 9.01, which would yield a gain of 0.56 unit instead of the 0.73 with that sample left in. When all 15 samples are averaged for chroma, the means are: 8.63, 8.74, 8.76, and 9.04. With the sample GBs2 omitted, the last mean would be 8.98, and the total gain would be 0.35.

This is surely not enough to warrant the use of the Munsell mask with its small rectangular window, for the purpose of controlling the size of color samples within the range of 0.25 to 16 square inches.[5] It means that under our conditions the effect of increased size has about reached its limit. The darker colors, those of low degree of illumination did seem to gain more in chroma. Is this due to lower initial chroma or to lower illumination? It might be that any color of low original saturation would gain more by increased size. All the darker colors were poorly saturated as compared with the colors of medium tint. But on the other hand, they were more saturated, on the average, than the colors of lightest tint. All the colors of medium tint were rather highly saturated to begin with. It would be interesting to know what would have happened had they been very poorly saturated. For a solution of the more general problem a rather extensive variation in the stimuli must be used.[6] For the usual condition under which the *Munsell Book of Color* is used, however, we believe that we have demonstrated that for small samples of colored material from ½ to 4 in. in diameter, size is not an important factor.

[5] The mask would still serve its function of isolating the colors to be matched and of giving them a uniform background where a uniform background is not otherwise provided.

[6] In a communication from Mr. A. E. O. Munsell I am told that very large areas do seem more highly saturated. He says, "On very large areas there is clear evidence of an increase in chroma of between one and two and one-half steps." To cite a particular case, a pigment which in small samples rated G 6/4, in an area of 80 sq. ft. rated G 6/6. Gray had to be added in order to reduce the rating to G 6/4.

IX. Personality Traits and Fluctuations of the Outline Cube

By Norman O. Frederiksen and J. P. Guilford

McDougall[1] has suggested that the rate of fluctuation of an ambiguous figure, such as the outline cube, might be used as an objective test of introversion-extraversion (I-E). The theoretical basis for this suggestion is that the traits of I-E are dependent upon a hypothetical "secretion X" whose function is to counteract the inhibitory effect of the cortex over the lower centers.[2] Individuals who are most extravert have this secretion in greater abundance; introverts are more deficient in this secretion. The secretion operates by increasing the synaptic resistance in the cortex, which affects the rate of change of an ambiguous figure; for introverts the cube will shift rapidly and for extraverts slowly.

This hypothesis has been subjected to experimental tests in the laboratory at the University of Nebraska[3] and by Professor Washburn[4] at Vassar. Very little has been found to give support to the theory, and yet the theory has not been conclusively disproved. Correlations between scores on the Marston and Neymann-Kohlstedt tests and the rates of fluctuation of the cube are statistically insignificant. It is a well-known fact, however, that the typical paper and pencil tests of I-E do not agree very well among themselves. It was one purpose of this study to use a better criterion of I-E and to examine a larger number of *Ss* than had hitherto been employed.

Certain earlier findings have even been encouraging enough to promise positive results more favorable to the theory. Guilford and Braly[5] found that the effect of strychnine is to increase the fluctuation rate, which is in agreement with McDougall's theory, since strychnine is supposed to reduce resistance at the synapses. Guilford and Hunt[6] found a correlation of 0.44 between fluctuation rate and the Laird test, a coefficient which was about 4.5 times its probable error. They also found some indication of a negative relationship between extent of knee-jerk and fluctuation rate, which would be expected if the latter measures the degree of cortical dominance over the lower centers. Hunt and Guilford[7] found in a later study that the average fluctuation rate for dementia praecox patients was about

[1] Wm. McDougall, *Outline of Abnormal Psychology*, 1926, 444.

[2] McDougall, The chemical theory of temperament applied to introversion and extroversion, *J. Abn. & Soc. Psychol.*, 24, 1929, 293-309.

[3] J. P. Guilford and K. W. Braly, An experimental test of McDougall's theory of extroversion-introversion, *J. Abn. & Soc. Psychol.*, 25, 1930, 382-389; also J. P. Guilford and J. M. Hunt, Some further experimental tests of McDougall's theory of introversion-extroversion, *J. Abn. & Soc. Psychol.*, 26, 1931, 324-332.

[4] M. F. Washburn, et al., Experiments on the relation of reaction-time, cube fluctuation, and mirror drawing to temperamental differences, this Journal, 41, 1929, 112-117.

[5] Guilford and Braly, *op. cit.*, 388.

[6] Guilford and Hunt, *op. cit.*, 329.

[7] J. M. Hunt and J. P. Guilford, Fluctuations of an ambiguous figure in dementia praecox and manic depressive patients, *J. Abn. & Soc. Psychol.*, 27, 1933, 443-452.

4 times that of manic depressives. If these pathological types are admitted to be the extremes of I-E, this fact further supports McDougall's theory.

By way of securing an improved criterion of I-E, Guilford and Guilford[8] have constructed and statistically analyzed what they consider to be a typical test of I-E of the questionary type. Thirty-five items were selected from a tabulation of the terms describing extraverts and introverts in the writings of recognized authorities on the subject, and from tests and rating scales in actual use. These items were selected on the basis of greatest frequency and of minimal overlapping, and were incorporated into a test. The 35 items are presented in Table I in the form of brief descriptive phrases. The exact form of wording of each item as it appears in the questionary may be found in the study just referred to. The test was given to nearly a thousand *Ss* and the data were analyzed according to Thurstone's[9] method of multiple factor analysis. By this method a surprisingly large number of factors were revealed in the test, 4 of which were thought significant enough to be described. Three of these 4 were identified as (1) *a tendency to shrink from the environment;* (2) *emotional sensitiveness;* and (3) *egocentrism.* They were found to be very closely related to one another, giving the appearance of one more inclusive variable. The fourth factor was called *impulsiveness,* which seems to correspond more closely to McDougall's conception of I-E. It is apparently independent of the other 3 factors, and it would be almost completely obliterated in the total score on the usual I-E questionary. A further purpose of the present study was to test the possibility that this factor of *impulsiveness,* or any other group factor within the test, correlates with fluctuation rate. In order to touch any possible personality trait which is represented in the typical I-E questionary, we correlated the fluctuation rate with every item separately, as well as with the total score.

The total score in the Guilford questionary was based upon differential weightings[10] for "Yes" and "No" responses to each item. These weightings were obtained by an application of Thurstone's[11] method of similar reactions to the data obtained from 930 *Ss.* In the present study, data were obtained from 95 *Ss,* all university students. As usual, self-ratings were secured, and in addition the ratings of two associates, close friends or relatives, on every *S.* The reliability of the questionary, used as a self-rating device, has been found to be 0.84, test-retest, with 120 *Ss.* The correlations between self-ratings and associate-ratings in this study were $0.482 \pm .053$ and $0.586 \pm .048$, the two different associate-ratings for each *S* being correlated separately. The correlation of the two sets of associate-ratings was $0.609 \pm .046$. The correlation of the self-ratings with the means of the two associate-ratings was $0.618 \pm .045$. It would appear that the associate-ratings are about as valid as the self-ratings, and so they were given equal weight with the self-ratings in pooling the 3.

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[8] J. P. Guilford and R. B. Guilford, An analysis of the factors in a typical test of introversion-extroversion, *J. Abn. & Soc. Psychol.,* 29, 1934 (in press).

[9] L. L. Thurstone, Multiple factor analysis, *Psychol. Rev.,* 1931, 38, 406-427.

[10] J. P. Guilford, An examination of a typical test of introversion-extroversion by means of the method of similar reactions, *J. Soc. Psychol.,* 4, 1933, 430-442.

[11] L. L. Thurstone, Theory of attitude measurement, *Psychol. Rev.,* 36, 1929, 222-241.

The 95 *Ss* observed the outline cube for a total time of 30 min., this time being divided into 3 sets of observations made on 3 different days. Guilford and Hunt[12] have found that a very reliable measure of fluctuation rate can thus be

TABLE I

THE TEST ITEMS AND THEIR CORRELATIONS WITH FLUCTUATION RATE AND WITH THE KEY ITEM FOR THE IMPULSIVENESS FACTOR

	Meaning of the item	r_{kf}*	r_{k30}†
(1)	Expresses self better in speech than in writing	.105	.157
(2)	Limits acquaintances to select few	.115	−.110
(3)	Prefers to take the lead in group activities	.052	
(4)	Prefers to read about a thing rather than to experience it	.157	
(5)	Likes detailed work	.042	
(6)	Particular about dress and personal property	.000	
(7)	Considerate of other people's feelings	−.051	
(8)	Inclined to act on the spur of the moment	.249	.348
(9)	Has kept a personal diary of own accord	−.088	
(10)	Works much better when praised	.093	
(11)	Likes to change type of work frequently	.020	
(12)	Inclined to study the motives of others	−.072	
(13)	Daydreams frequently	.018	.177
(14)	Prefers to work with others rather than alone	−.016	
(15)	Worries over possible misfortunes	.076	.171
(16)	Is frequently somewhat absent-minded	−.161	
(17)	Likes to persuade others to his point of view	.091	.254
(18)	Keeps in the background on social occasions	.122	−.284
(19)	More interested in athletics than in intellectual things	.048	
(20)	Dislikes to change opinions already formed	−.193	.216
(21)	Likes to speak in public	−.001	
(22)	Prefers to work things out in his own way	−.019	−.217
(23)	Has frequent ups and downs in mood	−.120	.463
(24)	Inclined to be slow and deliberate in movement	.140	−.281
(25)	Feelings rather easily hurt	.090	.306
(26)	Enjoys getting acquainted with most people	.097	.265
(27)	Inclined to keep quiet when out in company	.113	−.317
(28)	Adapts self readily to new conditions	.164	
(29)	Likes to confide in others	.001	.445
(30)	Expresses emotions readily	.053	
(31)	Thinks about self much of the time	−.053	.159
(32)	Likes to be watched while working	−.150	
(33)	Frequently rewrites social letters	.289	
(34)	Likes to sell things	−.062	
(35)	Gets rattled easily in exciting situations	.025	.200

* r_{kf} refers to the correlation between each item and the rate of fluctuation of the cube. The probable errors of these coefficients range from 0.063 to 0.069.

† r_{k30} refers to the correlations between item 30 and other items, only those coefficients more than 5 times their probable errors being given.

obtained. The number of changes per min. was taken as the fluctuation rate. Other conditions were almost exactly the same as in the study just referred to. *S* was instructed to take a passive attitude, since this has been found satisfactory.

The mean of the rates was 21.6 with a standard deviation of 12.8, and the range was from 4.6 to 74.4 per minute. The inter-daily correlations of the rates were: days I and II, 0.894 ± .017; II and III, 0.897 ± .016; I and III, 0.713 ± .041.

[12] Guilford and Hunt, *op. cit.*, 328.

When the three days' measures are pooled the reliability is much higher. Previous studies give a reliability coefficient (odds-evens) in the region of 0.96 to 0.98 for this test. A test with such high reliability and such a wide range of measurements must be a delicate indicator of some constant factor or set of factors in the individual. Studies now in progress aim to discover what some of the factors are.

Now for the crucial test, the correlation between fluctuation rate and I-E test score. Using the self-ratings alone, as has been done in the past, the correlation is only $-0.047 \pm .070$. Using the pooled self- and associate-ratings, the correlation is $0.066 \pm .070$. This verifies the lack of relationship shown by previous studies, and it is more decisive because of the improved conditions of this study. We are even encouraged to generalize, and to maintain that there is probably no relationship whatever between the rate of fluctuation of an ambiguous figure and any questionary on I-E of the usual type.[13] The only loophole that we can now see is that some other fluctuating figure, perhaps even McDougall's windmill, might give positive results. Such results would mean that there is little or nothing in common between the fluctuating cube and the windmill, a very improbable event. The relationships between these two phenomena and between several varieties of fluctuation are now being investigated in our laboratory.

The next and final step was to compute coefficients of correlation between fluctuation rate and the individual test items.[14] Each test item was evaluated for each *S* by summing the 3 weights belonging to the judgments of the 3 raters. The correlations are given in Table I under the caption r_{kf}. According to the theory, these coefficients should all be negative, since the higher the weighting, the more extravert the individual on each item, as I-E has been traditionally conceived and as the statistical weighting of Guilford's study dictated. The highest coefficient, either plus or minus, is just 4.6 times its probable error, and only one other is anywhere near that size. One item—(33), Do you frequently rewrite social letters before mailing them?—correlated with fluctuation rate to the extent of $0.289 \pm .063$, and another item—(8), Are you inclined to act on the spur of the moment without thinking things over?—to the extent of $0.249 \pm .065$. *But both of these correlations are positive while McDougall's theory calls for negative relationships.*

Let us now examine the correlation between rate and those items which contain the impulsiveness factor. The key item, *i.e.* the one most heavily loaded, with the impulsiveness factor, is number (30)—Do you express such emotions as delight, sorrow, anger, etc., readily? The correlation between this item and fluctuation rate is $0.053 \pm .067$, indicating no relationship. Those items which correlate relatively high with item (30), either positively or negatively, are probably loaded to some extent with the factor of impulsiveness. There are 24 such items. None of these correlations are high, to be sure, but their probable errors are in the region of ± 0.02, so they have some meaning. In the column headed r_{k30} we have given the correlations

[13] This conclusion is further supported by the fact that one *S*, not included in the correlations, who had a fluctuation rate of 144.5 shifts per min., was just about average, or perhaps slightly on the extravert side on the questionary. Her abnormally high rate was verified 6 mo. later by another experimenter. Her outstanding temperamental trait as described by the experimenter was extreme vivacity, but not necessarily impulsiveness.

[14] The reliabilities of the individual test items have been found to range between 0.60 and 1.00, with 277 *S*s (see reference in footnote 8).

between those 24 items and item number (30). If McDougall's theory were true, we should expect to find a significant negative correlation between item (30) and fluctuation rate. Whenever any item containing the impulsiveness factor correlates positively with item (30) it should correlate negatively with the fluctuation rate, and vice versa, and the amount of correlation should be in proportion to the saturation of the item with the impulsiveness factor, as indicated roughly by the correlation of the item with number (30). None of these requirements prove to be true. In 12 of the 24 cases there is a reversal of sign, and in 12 cases the signs are the same. The two items, (23) and (29), which correlate highest with item (30) correlate with fluctuation rate to the insignificant extent of —0.120 and 0.001.

One might in a similar way show that none of the other group factors bear any relationship to the rate of fluctuation of an outline cube, but it seems useless to do so, since the degree of correlation of the individual test items with the rate is so small in all cases. If we find no correlation with any of the traits which are expressed in the test questions, surely no larger factor made up of these traits will show any such relationship. The correlations of fluctuation rate with the key items of the other large common factors are only 0.122, 0.025, and —0.053, for items (18), (35), and (31) respectively. Surely none of the 4 main factors apparent in the typical I-E questionary are related to fluctuation rate.

Conclusions

(*1*) The results of previous experiments which indicate that the rate of fluctuation of an ambiguous figure cannot be used as an objective test of introversion-extraversion are verified by this study.

(*2*) Correlation of each individual item of the typical test of introversion-extraversion with fluctuation rate shows that with one possible exception no relationships exist between these measures.

(*3*) Examination of the correlations for the test items containing the factor of *impulsiveness* reveals that this factor does not bear any relationship to fluctuation rate.

(*4*) It is clear that no group factors residing in the typical test of introversion-extraversion bear any significant relationship to fluctuation rate, since no single items correlate with rate.

APPARATUS

DEMONSTRATION UNIT FOR PATTERNS OF APPARENT MOVEMENT

By E. Lowell Kelly, Connecticut State College, and W. R. Miles, Yale University

It is a relatively simple matter to give a demonstration of apparent movement for a large or small group of students by alternately flashing two lights so placed that each throws its shadow of some opaque object at a different place on a wall or screen. A more interesting demonstration may be given, however, if the arrangement is such that the seen movement can be integrated into various patterns. The unit described here makes such versatile demonstrations readily possible.

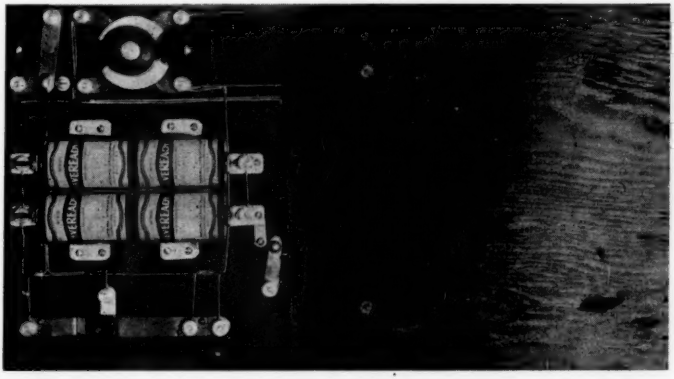

Fig. 1. A Self-Contained Portable Unit for Demonstrating Patterns of Apparent Movement

As an apparatus for demonstrating *phi* phenomena, this unit is fairly simple to construct and offers relatively little to get out of order. It can be used in large or small classes without darkening the room or making any other special arrangements. By reason of its compactness, the lecturer or experimenter can easily hold it so as to give all the observers in turn optimal conditions for observation. The basic ambiguity of some of the integrated patterns of movement instantly brings out individual differences in judgment or impression, and so creates interest in the phenomena.

The arrangement and operation of the device will be fairly obvious from the illustrations, one of which is a photograph of the actual unit, and the other is the wiring diagram. Two wooden panels, one (the base) 25 by 44 cm., and the other (the field) 25 cm. square, are hinged together at the right-hand end, so that the

smaller panel, which in Fig. 1 appears folded down, may be easily turned up at right angles to the baseboard and locked by a bracket hook. The base panel carries the batteries, clips and switches; the field panel provides mounting for 5 lamps arranged in a 15 cm. square, with one lamp at the center. Small flash-light bulbs are used, and flash-light cells serve as the source of power.

As will be seen on referring to the diagram, Fig. 2, the circuit is a simple one. The on-off switch A is in series with the battery and must be closed when the unit is in operation. B, is a selector switch, which makes it possible to flash the lights in pairs as follows: 1–4 and 2–3 horizontal pairs; and 1–3 and 2–4 diagonal pairs.

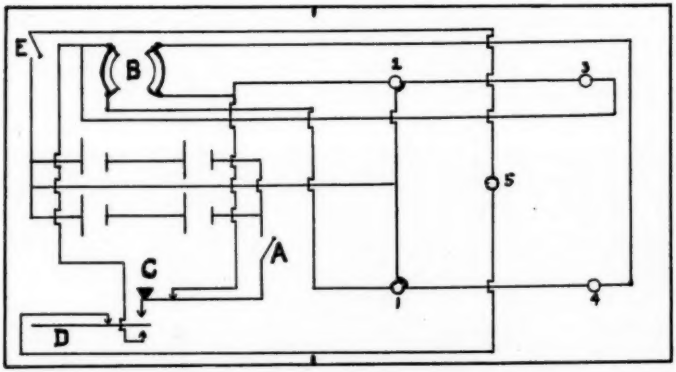

FIG. 2. DIAGRAM OF THE HAND KEY AND OF CIRCUITS FOR THE ALTERNATE FLASHING OF PAIRS OF LAMPS

The flashing of the lamps is controlled by the make-break key, C. When no pressure is on this switch one pair of lights remains continuously illuminated. A slight pressure serves to extinguish this pair of lights and more pressure on the key spring will close the lower pair of terminals and thus light the other pair of bulbs. An interval of about $\frac{1}{15}$ sec. is optimal between the flashing of the pairs of lights but after observers have "seen" the movement, the rate can be varied through quite a range without causing the movement to disappear. Thus flashing the lamps by hand is satisfactory for demonstration purposes. When it is desired to use the center light, switch E is closed. Then the middle light is turned on and off in the mid-portion of the movement of the control button C.

The demonstration may well begin by the use of the diagonal pairs and without including light no. 5. If the unit is rested on a horizontal table, alternately flashing 1–3 and 2–4 will not only give the appearance of movement to the spectators who are sitting eight feet or farther away, but this movement, in most all cases, will assume one of two patterns: (a) reciprocal movement up and down and as a total resembling a seesaw; or (b) horizontal movement, the pattern giving the appearance of a double pendulum swinging back and forth about the unilluminated

light no. 5 as a fulcrum.[1] Probably 95% of the spectators will see one or the other of these things happen, even though the light bulbs themselves are in clear view. Some *O*s report that the motion follows curved paths. Others are equally impressed with the straightness of the lines between the terminal positions.[2] As a rule, the *O*s cannot readily make a shift from a vertical to an horizontal movement configuration, *i.e.* from the seesaw to the pendulum, or in the opposite order, but if they continue to view the field for some time, the pattern will shift back and forth as in illusions of reversible perspective. Our preliminary data indicate that the seesaw is usually observed first when the four lights are equidistant and tends to dominate as a pattern. We will not undertake to discuss this finding at present.

The experimenter may hold the apparatus in his hands and, as he flashes it, rotate it on an axis perpendicular to the plane of the lamps by turning the entire base panel up on edge. Under these circumstances, if the *O*s maintain their respective vertical or horizontal movement patterns, these patterns should be exchanged by the shift in the position of the apparatus.

Covering one of the corner lights will, as a rule, destroy the seesaw or the pendulum interpretation and cause the substitution of another in which both vertical and horizontal movements appear. As an example, if light no. 1 is removed or covered, extinguishing the 2–4 pair and illuminating 3 will give the appearance that lights 2 and 4 suddenly concentrate in 3 and then 3 divides into its original components again. Similarly when the apparatus is rotated 45°, so that diagonal pairs assume vertical and horizontal positions respectively, flashing these pairs ordinarily gives no impression of a rotating pattern, but rather the appearance of movement toward the center of the field and then at right angles towards the periphery.

Having observed the appearance of a rotary pattern, it is very easy for the usual observer to get the impression of horizontal line movement from the left hand pair 1–4 to 2–3 at the right and back and forth. If the apparatus is rotated this "bar movement" will occur vertically instead of laterally.

The use of light no. 5 flashed between two pairs complicates the movement. The impression of the bar or straight line is destroyed, and each pair in turn seems to concentrate in the middle member and then to radiate from it.

A similar apparatus has been used for the purpose of gathering data in reference to the frequency with which different patterns are seen by naïve and practiced *O*s and the length of time that one pattern of interpretation persists before another takes its place. Although manual operation of the key is fairly adequate for class demonstration, we have found it desirable, for experimental work, to incorporate a mechanically operated switch for accurately controlling the interval and rate of flashing. A metronome with mercury contacts or a motor driven commutator may be readily substituted for the key, but either serves to reduce the portability and versatility of the unit.

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[1] Occasionally the pattern of a border moving clockwise or counterwise is seen.

[2] If ground glass or a sheet of tissue paper covers the lamps the movement will, of course, be a little more compelling. This, however, is not in the least necessary. Even though the bulbs are in full view, the illusion of movement is extremely strong. In fact, one undergraduate student, after seeing the demonstration made the following entry in his notebook: "The instructor connected small flashlight bulbs to a metronome, which moved back and forth."

MIRROR TONOSCOPES

By Milton Metfessel and Harrison Musgrave,
University of Southern California

Recent developments in stroboscopy[1] and in radio technique[2] have shown the way toward new types of tonoscopes. We shall describe two models. The feature distinguishing them from other pitch- and speed-indicating devices is an electrically-driven vibrating mirror. The vibrating mirror yields the stroboscopic effect in lieu of a flashing light. A series of delineations passing the vibrating mirror, at the rate of one delineation for each full vibration, will come into full view. By using a mirror instead of a neon bulb, the tonoscope can be used under any lighting conditions. A dark or semi-dark room is not required.

Vibrating-mirror unit. The sound to be examined is directed into a microphone. From there it is amplified by an especially designed voltage amplifier. The output from the amplifier is fed into two rochelle-salt-crystal motors,[3] wired in parallel. The crystals of Rochelle salt bend from their normal plane when a voltage is applied to their surfaces, in the same way that quartz does. Their torque has considerable force. The bending of the crystals is used to move lever-arms on the front face of the crystal motors, and the lever-arms are in turn attached to the upper edge of the mirror (1 x 10 in.) to be vibrated. The mirror is supported along the center of its long axis, so that it moves with a rocking circular motion. The attachment of the lever-arm of the crystal motor to the mirror's upper edge is not rigid. A small piece of brass tape supplies the needed flexibility.

The crystal motors have the characteristics of a capacity of about 0.013 μf, rather than that of an impedance such as conventional loud-speakers have. For this reason the output of most commercial amplifiers is not suited to the crystal motor. A power-amplifier causes the speaker to "howl" from mechanical and electrical feed-back. A voltage amplifier of from a 50- to 200-volt swing on the output (and no direct-current component) is more efficient. The accompanying diagram (Fig. 1) shows a simple amplifier with its constants which has operated the mirror unit satisfactorily. As in most laboratory work, a battery set is better than one operated by A.C. current. The first tube, together with its input and output transformers, should be completely shielded from the rest of the set, in a separate metal box, because the "57" tube is extremely sensitive. All of its leads should be as short as possible.

The rochelle-salt crystals have great power, but they are brittle. Care should be exercised not to move or jar the driving lever unduly, for the corner of the crystal to which this arm is fastened may break off inside the housing. If a crystal is broken, the cover of the housing may be removed and the drawing finger cemented to the opposite corner of the crystal.

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[1] M. Metfessel, Stroboscopy by means of impressed eye movements or mirror vibration, *Science,* 78, 1933, 416-417.

[2] C. B. Sawyer, The use of rochelle salt crystals for electrical reproducing and microphones, *Proc. Inst. Radio Eng.,* 19, 1931, 2020-2029.

[3] Crystal motors may be secured from either the Brush Development Co., 3715 Euclid Ave., Cleveland, Ohio, or the C. H. Stoelting Co., 424 N. Homan Ave., Chicago.

Model A. A laboratory set-up of the large model is shown in Fig. 2. In addition to the vibrating-mirror unit there is a drum (11 x 10 in.) turned by an electrical phonograph-motor of variable speed. There are 65 rows of sine waves inscribed on the face of the drum. When revolved at 78 r.p.m., the range of the number of sine waves in each row passing a given point in a second is more than one octave (from 126.7 to 265.1). Each successive row increases with a value equivalent to one-tenth of a step of the tempered musical scale. For the details of this scale the reader is referred to Seashore's description of his neon-light tonoscope.[4] The 65 rows on the drum of the mirror-tonoscope are grouped into 13 units of 5 rows each. The middle row in each group is the value of the note on the scale (C, C♯, D, etc.). The scale

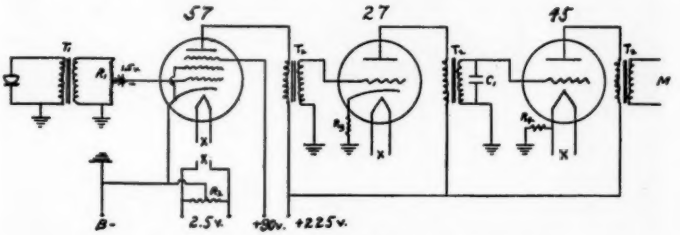

FIG. 1. AMPLIFIER FOR MIRROR TONOSCOPE

T_1 = Microphone input transformer (Kenyon)
T_2 = 3 to 1 audio-transformer (Amertran)
C_1 = 0.01 microfarads
R_1 = 250,000 ohm potentiometer
R_2 = 50 ohm center tap resistor
R_3 = 3000 ohm biasing resistor
R_4 = 1250 ohm biasing resistor
M = output to crystal motors

progresses straight across the drum, instead of doubling back on itself. We have found that the plan of grouping into 5 rows has certain perceptual advantages. A total pattern of stroboscopic movement in five rows makes it easy to find the single row with the smallest amount of movement.

In singing a note, then, the singer attempts to make the sine waves in the middle row appear to stand still. In case of failure, the number of tenths of a step he is sharp or flat can be noted by determining which row to the right or left of the middle appears to be stationary. For the octave above the range of sine waves, C_2 to C_3, the notes appear as a chain. This permits an easy identification of the right octave.

To make these sine waves, we developed a neon-tube oscillator. (Neon tube, variable condenser, resistance, and B batteries.) The frequency of this oscillator may be controlled by varying the condenser capacity. The output was amplified by one tube, and fed into the moving coil of a dynamic speaker on which a mirror was mounted. The field of the speaker was excited by a B battery. A wooden drum was mounted on top of the Seashore tonoscope, turning with it. Strips of 35 mm. film were wrapped around the wooden drum. Then, using the light of the neon bulb of the oscillator to illuminate the dots of the tonoscope, the oscillator was timed

[4] C. E. Seashore, The measurement of pitch intonation in singing and playing, *Univ. Iowa Stud.: Ser. on Aims and Progress of Research*, No. 172, 1929, 29.

stroboscopically so that its frequency matched the row of dots to be photographed. A light beam was arranged with a shutter so that the light would fall on the dynamic speaker's mirror for exactly one revolution of the tonoscope and drum. Pressing a button did this automatically. When the oscillator was timed to the row to be photographed, the button was pressed. This exposed the moving-film strip to the

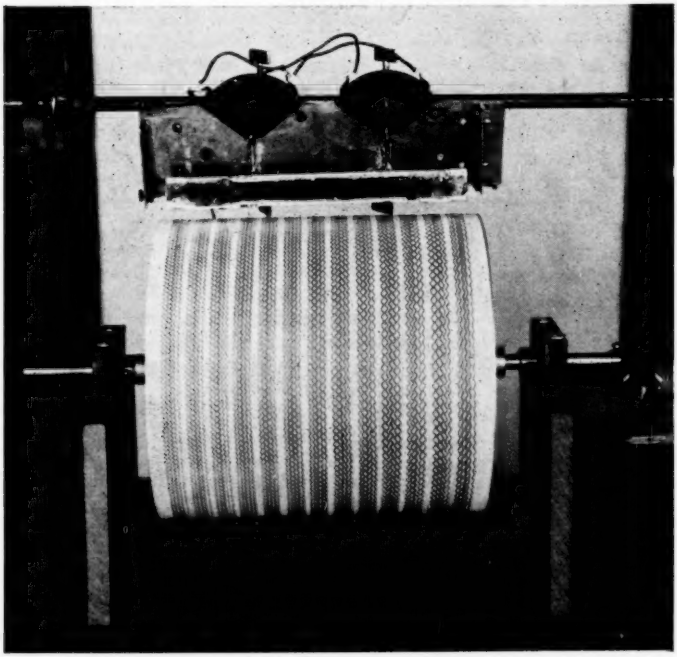

FIG. 2. MIRROR TONOSCOPE

oscillating light line reflected from the vibrating mirror on the dynamic speaker. Five wave trains were photographed on each film strip.

One large negative was made by photographing all 13 strips together, so that prints can be made from the final negative with little cost.

Model B. The vibrating-mirror unit of this simpler model consists of the same amplifier as Model A, but has only one crystal motor which vibrates a mirror 1 x 4 in. Instead of a drum with 65 rows of sine curves, there is a disk 12 in. in diam. which is placed on a phonograph turn-table. The disk has 12 annular rings with the number of radial lines in each ring equivalent to the half-steps of the tempered musical scale (international pitch), when revolved at 80 r.p.m. A picture of the disk may be found in a previous article.[8]

[8] M. Metfessel, *Science*, 78, 1933, 417.

INSTRUMENT FOR MEASURING THE SERIAL REACTION-TIMES OF LARGE MUSCLE GROUPS

By MALCOLM CAMPBELL, New York University

The apparatus described here permits the measurement of serial reaction-times from large muscle groups such as in the arm, the leg, the neck, or trunk. It is so constructed as to minimize preëxperimental differences between musculatures in accuracy of movement.

The reaction-block (Fig. 1) is diamond-shaped having an aperture in the center

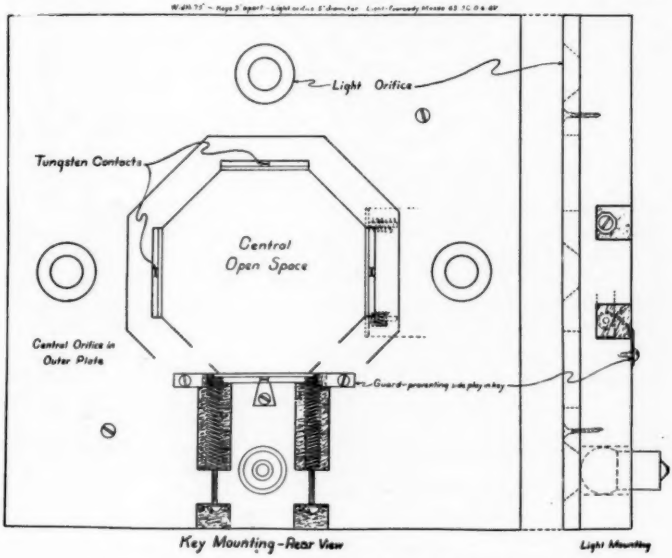

FIG. 1. STIMULUS-UNIT FOR HAND AND FOOT REACTIONS

and a small light at each of the four corners. These lights supply the stimuli. *S* reacts to them by thrusting a short baton through the aperture in the reaction-block and striking the key adjacent to the illuminated bulb. Closing this contact causes the light to be turned off at the same time turning on another. *S*'s task is one of pursuit in which he sets his own speed. Two factors operate to minimize the factor of accuracy as differential between musculatures. First, the four contacts are within an inclosed space which is small enough so that *S* need never move his baton more than 3 in. to reach a target. The targets or contacts are themselves $1\frac{1}{2}$ in. wide so that they are difficult to miss. Secondly, each target is in a corner of the diamond-

shaped aperture or inclosed space. An inaccurate stroke would glance off the side of the aperture onto the key.

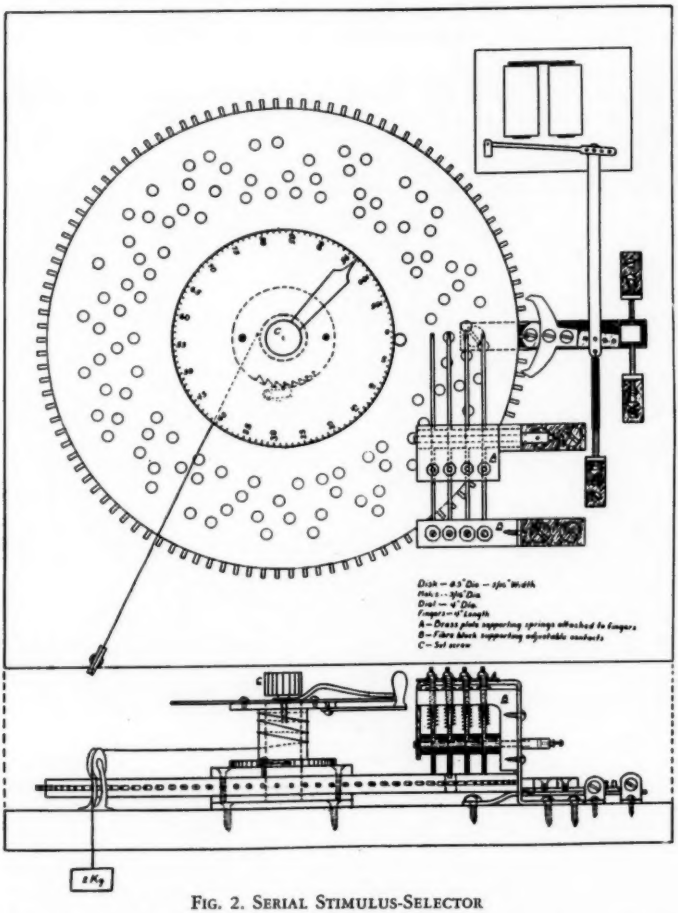

FIG. 2. SERIAL STIMULUS-SELECTOR

An essential supplement to the reaction-block is the stimulus-selector (Fig. 2) which makes the presentation of stimuli automatic and in random order. The principal feature of this apparatus is a fiber disk which is mounted on an axle and is energized by a weight attached to a string wound around the axle. Into the edge of this disk are driven, at equal distances, 110 pegs. A clock-work escapement mecha-

nism engages these pegs and is itself operated by the arm of a small door-bell magnet. *S*'s baton, in striking a target in the reaction-block, closes the circuit through the magnet which operates the escapement thus allowing the disk to revolve as far as the next peg. The movement of the disk in turn causes the magnet circuit to be opened again.

The selection of stimuli is the function of 4 metal fingers which are mounted on an axle over the disk. A small coiled spring attached to one side of each finger causes the tip of the other side to rest on the disk. As the disk revolves the 4 fingers trace concentric circles on its surface. On the surface of the disk are 110 radii, one for each peg on the circumference. On each radius a hole is bored through the disk. This hole is bored at the point where the radius intersects one of the concentric circles traced by the 4 fingers. For successive radii the holes are through different circles so that the order of succession is random between the 4 circles. At any point of rest of the disk the tip of one metal finger protrudes slightly below the disk's surface into a hole. The other end of the finger is correspondingly raised and a contact is made there which closes the circuit for one of the lights in the reaction-board. When any finger tips are not protruding into a hole of the disk, of which there is but one per radius, the contacts on their opposite ends are broken and the corresponding lights are out. Thus, one light appears to *S* at a time and the succession is random through 110 reactions before repetition begins. This is ordinarily too long a series to be learned in a few trials. Changing the connections of two or more fingers would institute a new pattern in case learning is suspected.

A mechanical counter may be introduced by numbering the radii of the disk and observing how many pass a fixed point during a trial. In operation with the arm the baton is simply held in the hand. No wires are necessary in connection with the baton. When the leg is employed *S* may be seated on a stool high enough so that the leg may swing freely from the knee and a reaction-block mounted in front of his foot. A baton is fixed to a device similar to a skate with the runner or rollers removed and is strapped firmly over the shoe.

A LECTURE-ROOM DEMONSTRATOR OF THE VISUAL TAU EFFECT

By Edward W. Geldreich, University of Cincinnati

Aside from its actuality as a psychological experience, the Tau Effect is a lecture-room curiosity by virtue of its efficacy in demonstrating the actual dependence of space (perception) upon time.[1] Thus, if three discrete points of light are successively presented, and the distance between the points of light is equal, but the time interval between the first and second points of light is greater (lesser) than the time interval between the second and third, then the perceived space between the first and second will be greater (lesser) than the perceived space between the second and third points of light. To the psychologist, this phenomenon offers a multitude of problems, and forces the factor of time before his notice as a factor not to be

[1] H. Helson and S. M. King, The tau effect: An example of psychological relativity, *J. Exper. Psychol.*, 14, 1931, 202-217.

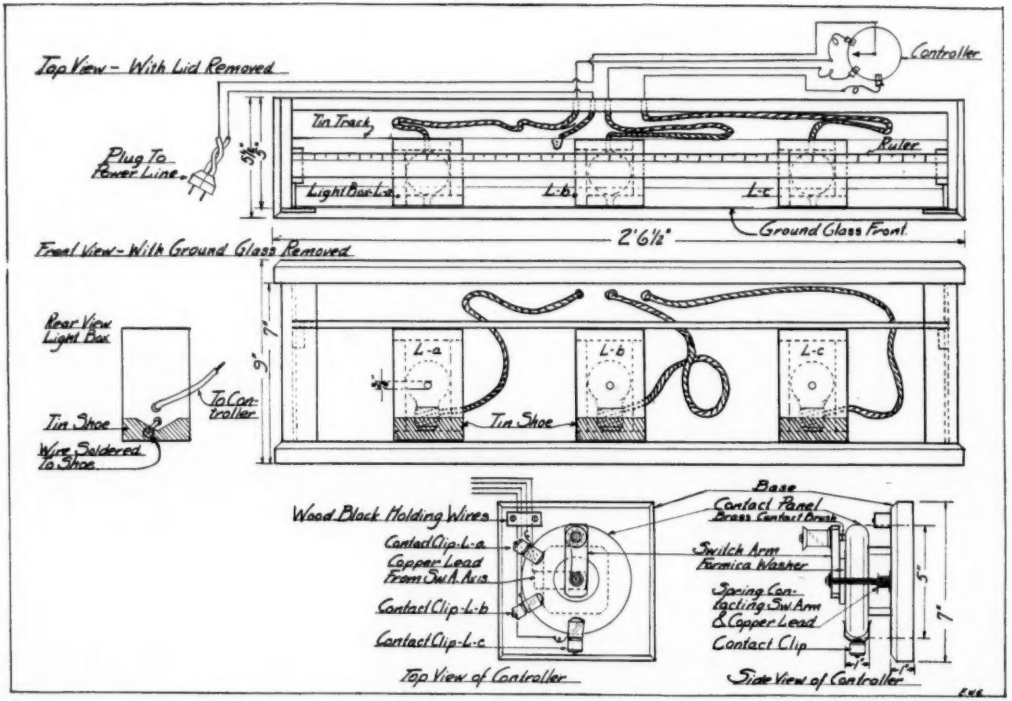

FIG. 1. DEMONSTRATOR OF THE VISUAL TAU EFFECT

slighted in any theoretical consideration of perception; but to the layman, the physicist, and the relativistic philosopher, the Tau Effect remains a curiosity and something to talk about. The apparatus here presented is a lecture-room demonstrator of the visual Tau Effect.

The demonstrator consists of a rectangular cabinet with a ground glass front and a lid, three light boxes each containing a 15-watt, 110-volt frosted lamp, and a manual controller made up of a revolving switch arm, three removable and adjustable contact clips, and a circular contact panel. (See Fig. 1.)

Construction features of the demonstrator are: (1) a tin track upon which the three light boxes ride, so as to make contact with the track by means of tin shoes, thus establishing a common return for the circuit; (2) a piece of glazed, 6-in. polygraph paper, pasted on the back of the ground glass front of the cabinet to obliterate the 'shadow-like effect' of the light boxes through the glass when just the ground glass is used; (3) a freely moving switch arm (its circuit is completed by means of a copper lead, a spring making contact with the copper lead and the base of the switch-arm axis, a metal washer at the other end of the switch-arm axis, a soldered wire from this washer to the brass contact at the distal end of the switch arm which in turn makes contact with the clips); (4) home-made brass clips bent just enough to pinch the contact panel firmly; and (5) a ruler running the full length of the cabinet against which the distance between the points of light can be read.

The demonstrational features of the apparatus are: the possibility of demonstrating the effect in a lighted lecture room, the ease with which the time ratio between the successive occurrence of the points of light can be changed, simplicity, freedom from the possibility of suggesting deception, and ruggedness of construction. The apparatus may be used also to demonstrate the phi-phenomenon.

FIG. 1. DEMONSTRATOR OF THE VISUAL TAU EFFECT

NOTES AND DISCUSSIONS

Faculty Psychology and Personality Traits

Noteworthy is it that long after a psychological concept has proved untenable and has been ostensibly abandoned, it often survives in disguised form. For example, in spite of the fact that psychologists generally have rejected the term 'faculty' and the concept for which it stands, some investigators continue to speak and to write in a manner which suggests that they are still thinking in terms of a faculty psychology. In *The Commonwealth Teacher-Training Study* Charters and Waples present a list of 83 'traits' judged by competent critics of teachers to be extremely important.[1] "Competent" individuals were first interviewed to discover what they believed to be the traits most essential for success in teaching.[2] These individuals explained the meaning of each trait by giving examples (so-called trait-actions) wherein each trait was exemplified. Traits and trait-actions were matched in the manner following:

"When the interviews were completed and the materials compiled in preparation for the next step, the result appeared as a list of trait names used by the persons interviewed and a list of several hundred trait actions, such as 'knows how to meet people,' 'endeavors to correct his faults,' 'keeps his records accurately,' 'does not shift responsibility,' 'helps willingly when asked to do so,' and so on. It then became the task of the translators, by consensus, to determine the traits expressed by the trait actions. The method of carrying out this process is described as translation."[3]

TABLE I

Trait-Actions Illustrative of "Adaptability"

1. Shows willingness to put up with a poor school system, and an unfriendly community.
2. Knows how to meet people.
3. Likes to be wherever he is.
4. Fits himself into the system.
5. Mingles with one class as well as with another.
6. Shows willingness to change method when he finds that the children are not being reached.
7. Becomes a part of the community in which he is teaching.
8. Shows the desire to adjust himself to new situations.
9. Does not make a fuss over irregularities of classroom procedure, when the room is cold, for example.
10. Meets the people of the community on their own level.
11. Does not dance or play cards if the community objects.
12. Shows willingness to readjust his theories and practices to existing needs and conditions.
13. Does not insist on having his class scheduled at a particular hour.
14. Moves about the place as if he belonged there.
15. Makes the best of every bad situation.
16. Observes local customs and conventionalities.

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[1] W. W. Charters and D. Waples, *The Commonwealth Teacher-Training Study*, 1929, 223 ff.
[2] *Op. cit.*, 15.
[3] *Ibid.*, 55.

When the translation was completed each of 83 traits was defined by a short list of trait-actions. Thus the investigators proceeded: (1) reports of experts or competents were employed to ascertain the characteristics of a good teacher; (2) the somewhat uncontrolled stimulation resulted in complex mutterings and confused descriptions such as "likes to be wherever he is" and "endeavors to correct his faults." These descriptive statements were labelled 'trait-actions;' and (3) the translators thereupon decided which of the descriptive phrases illustrated each trait. By this process of specious reasoning, "Shows willingness to put up with a poor school system" and "Likes to be wherever he is" were alleged to reflect 'adaptability.' Fourteen additional trait-actions, illustrative of 'adaptability' were set forth (see Table I). All appear so vague that it is doubtful that any other group of "competents" would associate the trait 'adaptability' with the descriptive phrases in this confusing miscellany.[4]

Because many of the 83 traits were nearly synonymous,[5] and because such a large number of traits is presumably unwieldy (for instructional purposes), the 83 were telescoped into 25 families of closely related traits. Each of these 25 families was assigned a general generic trait heading, followed by similar traits. The generic headings and the closely allied traits are presented in Table II. In Table II (Item D) the terms "carefulness," "accuracy," "definiteness," and "thoroughness" are to

TABLE II

FAMILIES OF TRAITS JUDGED TO BE EXTREMELY IMPORTANT BY COMPETENT CRITICS OF TEACHERS

A. Adaptability
B. Attractiveness (personal appearance)
C. Breadth of interest (interest in community, interest in profession, interest in pupils)
D. Carefulness (accuracy, definiteness, thoroughness)
E. Considerateness (appreciativeness, courtesy, kindliness, sympathy, tact, unselfishness)
F. Coöperation (helpfulness, loyalty)
G. Dependability (consistency)
H. Enthusiasm (alertness, animation, inspiration, spontaneity)
I. Fluency
J. Forcefulness (courage, decisiveness, firmness, independence, purposefulness)
K. Good judgment (discretion, foresight, insight, intelligence)
L. Health
M. Honesty
N. Industry (patience, perseverance)
O. Leadership (initiative, self-confidence)
P. Magnetism (approachability, cheerfulness, optimism, pleasantness, sense of humor, sociability, pleasing voice, wittiness)
Q. Neatness (cleanliness)
R. Openmindedness
S. Originality (imaginativeness, resourcefulness)
T. Progressiveness (ambition)
U. Promptness (dispatch, punctuality)
V. Refinement (conventionality, good taste, modesty, morality, simplicity)
W. Scholarship (intellectual curiosity)
X. Self-control (calmness, dignity, poise, reserve, sobriety)
Y. Thrift

[4] Evidence in support of this assertion follows in this paper.
[5] How, then, are we to know that there were 83 real traits?

be regarded not as synonyms but rather as a family [sic!] of closely related traits! The usefulness of the foregoing list is ingenuously described.

"Administrators, teachers, supervisors in teacher-training institutions, and the like will be able to use the evaluated list in a variety of ways. The instructor of each course may check off from the list those traits for which he will be responsible. Critic teachers in practice schools can use it as a basis for the development of traits in which the student is found weak. The list can be made the basis for direct instruction, either in classes or with individuals, and students can use it independently for self-evaluation."[6]

That this work of Charters and Waples has been considered seriously is reflected by the following quotation from Charters which Lomax has presented in *Commercial Teaching Problems.*

"Each student should be made aware of the *amount* of each trait he possesses, and in the case of traits in which an individual is weak, *exercises should be provided* by which he may *strengthen his control of the trait.*"[7]

No doubt educators would be extremely grateful if Charters and Waples (or Lomax) would define precisely how the teacher may determine "the amount" of each trait that the student possesses or should possess. Others would be interested in knowing something about the "exercises" by which the student may "strengthen his control of the trait." In addition, it is pertinent to inquire why 83 traits, if they are something more than nebulous verbiage, *can not* be used for instructional purposes. If we really know what traits and what amounts of each characterize a good teacher, we should capitalize this vital information.

Are the traits listed in Table II unitary? The foregoing suggestion of Lomax raises certain queries: (1) Is each of the 83 traits that are listed in Table II to be considered unitary? (2) If so, what is the difference between a 'personality trait' and 'a faculty'? Is it possible that the term 'trait' posits only that which was formerly designated 'a faculty'? Certainly, in their discussions of "traits" many modern writers (including Charters and Waples) do not indicate precisely what they mean by a "trait." It is true that in one of his books Charters defines the trait as a type reaction.[8] But the low intercorrelations which have been secured between various type reactions listed under the categorical heading "honesty" make us inquire whether the type reactions have an appreciable degree of objective reliability and validity. Certainly, the suggestion that exercises may "strengthen the control" of a trait is difficult to understand except in reference to 'faculties.'

The validity of the Charters-Waples proposal is without doubt open to serious question. Most modern psychologists agree with Thorndike, who in 1903 vigorously denied that man possesses "faculties."[9] In denying the validity of the faculty concept Thorndike described this concept as follows:

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[6] *Op. cit.*, 18 f.

[7] P. S. Lomax, *Commercial Teaching Problems*, 1929, 96. Italics ours.

[8] W. W. Charters, *The Teaching of Ideals*, 1928, p. 33.

[9] E. L. Thorndike, *Educational Psychology*. II. *The Psychology of Learning*, 1913, 363 f. Thorndike was, of course, not the first to refute the "faculty concept." Pratt has recently written as follows: "In the hands of Ch. Wolff and the later phrenologists the doctrine of faculties fell into disrepute, and the pride and delight which Herbart took in dealing it what he considered a death-blow is re-

"The mind is regarded as a machine of which the different faculties are parts. Experiences being thrown in at one end, perception perceives them, discrimination tells them apart, memory retains them, and so on. By training, the machine is made to work more quickly, efficiently, and economically with all sorts of experiences. Or, in a still cruder type of thinking, *the mind is a storage battery which can be loaded* with will power or intellect or judgment, giving the individual a surplus of mind to expend. General names for a host of individual processes—such as judgment,[10] precision, concentration—are falsely taken to refer to *pieces of mental machinery* which we can once for all get into working order."[11]

We have examined the trait-actions and the traits that are presented in *The Commonwealth Teachers-Training Study*. We believe that Charters and Waples are once more exemplifying a common human tendency; namely, equivocation or nominalism. The following describes the process of equivocation staunchly followed and defended by some character testers. Set up a name or label; measure or describe something else, assign the name or label to the thing measured or described. Charters and Waples of course did little or no measuring of the traits that are listed in their teacher-training study.[12] They simply attached descriptive labels to generic terms. This practice is a greater violation of common sense than that of the personality testers who measure, for example, the tendency of a child to indulge in overstatement, and then assert that the overstatement test is a fairly satisfactory device for gauging general character.[13] Equivocation is clearly evident in the interchangeable use of the terms; viz., general character and overstatement.

Attempts to deal with specifics. Small wonder that the student of contemporary psychology finds himself a bit confused when he reads the long list of 'personality traits' that have been spawned during the last two decades! Cogent criticisms of the so-called 'traits' are numerous. Link has described recently the extreme view of those who insist that generality characterizes personality traits. He writes as follows:

Most people believe that "if a man is lazy he *is* lazy. If he is industrious he *is* industrious. If he is cheerful he *is* cheerful. If he is disloyal he *is* disloyal. If he is ambitious he *is* ambitious.[14] If he is good he *is* good. And if he is bad he *is* bad. . . . Nothing could be farther from the truth. The moral qualities are not absolute. They are not blanket qualities which cover an individual's entire range of life no matter under what circumstances he may live. On the contrary, moral traits are *relative*, and their nature depends upon a very wide variety of external economic, social and bodily conditions."[15]

The foregoing assertion is substantiated in part by the work of May and Hartshorne. These investigators found that such a trait as 'deceit' is far from a unitary

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flected to-day in almost every textbook of psychology in those sections treating of theories once held by psychologists but now discarded." (C. C. Pratt, Faculty psychology, *Psychol. Rev.*, 36, 1929, 142.)

[10] Traits in this and in later quotations that appear also in the Charters-Waples list are identified by means of asterisks, see Table II, item K-1.

[11] *Op. cit.*, 363 f. Italics ours.

[12] In so far as the writers can discern by examining the published accounts.

[13] For criticism of this practice see the present writers' article, Further remarks upon character testing, *Psychol. Rev.*, 36, 1929, 91-95.

[14] See Table II, items N-1; P-3; F-3; and T-2.

[15] H. C. Link, *Employment Psychology*, 1919, 203 f.

entity. Indeed their work brings the very existence of this trait into question. Robinson has summarized their findings as follows:

"When Professors Mark May and Hugh Hartshorne combined thirty-two tests of deceit they found that the composite scores of the children to whom the tests were given represented nothing of any practical importance. The most likely conclusion seems to be that deceitfulness is not a single general trait analogous to intellect. There are instead many types of deceit varying with the objective circumstances. Some of these individual traits are clustered into minor general traits, but one grand general deceitfulness seems to be largely fictitious."[16]

Another criticism of character measurement takes a somewhat different approach. Hollingworth has insisted that certain so-called traits are not so much traits of one individual as traits of a pair of people, or functions of the particular circumstances in which the individual is placed.

"Many of these qualities or traits may be not so much traits of one individual as traits of a pair of people. A's coöperativeness[17] undoubtedly depends on the personality of B with whom he is coöperating. Should the resulting degree of coöperativeness be attributed to A or B? Apparently it should be applied to A and B as a team, rather than to either alone. Is a man's cheerfulness[17] a trait of the man or a function of the circumstances in which he is placed? We expect a stupid man to be quite generally stupid, and a musical person to be musical under all ordinary circumstances. Obviously coöperativeness and cheerfulness do not manifest themselves in this way, and such traits must be more closely related to circumstances and to social combinations of individuals than are the specific aptitudes and general competence."[18]

Hollingworth emphasizes a rather obvious fact; namely, that cheerfulness, coöperativeness, kindliness, and so forth, represent *effects produced;* he stresses also that these terms do not refer to psychological functions or basic elements of personality.[19]

One might continue almost indefinitely to cite quotations similar to the foregoing, but the field is familiar to the psychologist. Over and over, a battery of tests designed to measure traits such as persistence, or aggressiveness, or honesty, yields results so unreliable and undependable (when compared with other criteria) that one is led to question the actual existence of the general trait. In numerous instances the instruments are very loosely conceived and are clearly equivocal. A survey of the nature of a wide variety of frequently used tests suggests that there is a fundamental limitation common to most of them. Trait testers appear to assume that whatever they may *name* has objective reality; many need not so much to improve the measures but to improve or change their thought regarding 'traits.'[20]

Is the quantitative approach to the study of 'personality traits' a valid one? In 1914 Wells attempted a comparative study and a synthesis of the 'trait' schemes that had been proposed previously by Cattell, by Hoch and Amsden, by Heymanns and Wiersma, and by Davenport.[21] Wells listed 14 traits and about 95 sub-traits. He sought to justify his attempt by the fact that:

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[16] E. S. Robinson, *Man as Psychology Sees Him,* 1932, 249.
[17] See Table II, items F-1 and F-3.
[18] H. L. Hollingworth, *Vocational Psychology and Character Analysis,* 1929, 169 ff.
[19] *Op. cit.,* 145.
[20] Robinson, *op. cit.,* 253.
[21] F. L. Wells, The systematic observation of the personality in its relation to the hygiene of the mind, *Psychol. Rev.,* 21, 1914, 295-333.

"In the scientific study of the personality, the information must be cast in such a form as to give a judgment of the quantitative relationship of the subject to other individuals."[22]

Wells assumed apparently that science implies the quantitative approach; that we *must* use a quantitative method. Thorndike once stated that whatever exists exists in some amount and that "to measure it is simply to know its varying amounts."[23] McCall and others redacted this view, and Ruch indicated that he considered its acceptance basic in a program of educational measurement. Such statements may lead to invalid assumptions, since they fail to represent fairly the total situation. We simply do not possess adequate technique for making a quantitative study of personality. Indeed, many thoughtful workers have insisted that the quantitative study of an individual must always be accompanied or supplemented by qualitative evaluation (careful case-study) if useful results are to be secured.

Limitations of the quantitative procedure. Certain limitations of the quantitative approach are well illustrated in Wells' subjective analysis of traits.

"Traits are important not only for themselves but according to their combinations.[24] The same self-assertiveness, for example, that leads one individual to realize his aspirations, leads one less well endowed into situations he cannot cope with, thus strengthening the mental balance of the one, and undermining that of the other."[25]

One might mention also the trait (or is it merely a descriptive label?) "enthusiasm" which appears as item H in Table II. Enthusiasm is likely to be desirable when accompanied by (or combined with) high intelligence. Accompanying low intelligence or an unfavorable environment (or both), enthusiasm may resemble ingenuousness.[26] Since a characteristic such as enthusiasm never exists in isolation from total behavior patterns, it may or it may not be a desirable characteristic. The desirability or undesirability of enthusiasm depends obviously not only upon enthusiasm *per se* but also upon a host of concomitants. If enthusiasm *is* never an isolated or a detached phenomenon, one may logically ask: What is the best way of dealing with and directing desirable and undesirable manifestations of this aspect of behavior?

It is sometimes said that the educated, well-adjusted individual is characterized by a superior scheme of values. In other words, education and adequate adjustment imply useful and appropriate enthusiasm. An important difference between the educated person and the ignoramus is that the educated person's enthusiasm is evoked by worthy goals and aspirations. For attainment of these ends, enthusiasm is of course essential. Therefore merely to increase or to decrease enthusiasm might or might not be desirable. When change is expedient, reapportionment

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[22] Wells, *op. cit.*, 296.

[23] E. L. Thorndike, Measurement in education, *The Twenty-first Yearbook of the National Society for the Study of Education*, 1922, 9.

[24] William James and others had pointed to this fact previously. See James, *Talks to Teachers*, 1899, 114.

[25] Wells, *op. cit.*, 297.

[26] Trait terms such as 'ingenuousness' and 'enthusiasm' are linguistically or colloquially satisfactory and useful. However, in such usage, no scientific basis is assumed by us.

or realignment of enthusiasm is essential. Thus, the trait can not be considered in the quantitative way suggested by Charters and Waples. Surely, *it is enthusiasm as a part of a complex situation that is important!*

It will be recalled that in a foregoing quotation Charters and Waples suggested that the instructor may check off from Table II those traits for which he will be responsible. But, as has been emphasized by Robinson, "The more deep-seated traits are interwoven."[21] Moreover, as has been remarked by James, Wells, and others, it is *the combination* of traits that is really important in the highly integrated behavior of man. Do Charters and Waples consider "enthusiasm" to be an isolated, unitary, and quantitatively measurable entity which operates more or less independently and which can be "strengthened" by appropriate exercises? Do they assume that enthusiasm is a piece of mental machinery which we can once for all get into working order? The writings of these men suggest affirmative answers to the foregoing questions.

Charters and Waples, by means of an elaborate and expensive technique, attempted to define certain traits objectively. One is of course led to inquire whether the 83 alleged traits have any corporate existence; whether they are not simply verbalizations that are useful socially, but very misleading when an attempt is made to employ them scientifically. To the present writers it is certainly doubtful that these abstractions can be treated objectively; and serious question may arise regarding whether Charters and Waples have disentangled the categorical patterns sufficiently to establish and disclose the elements.

TABLE III

TRAIT-ACTIONS ILLUSTRATIVE OF CERTAIN OF THE TRAITS LISTED IN TABLE II

1. Settles things once for all when he does take an issue with a pupil.
2. Effectively prepares pupils for the next grade.
3. Overlooks the deficiencies of a suburban community.
4. Makes people feel that he knows his subject.
5. Gives up preconceived notions.
6. Does not make threats which cannot be carried out.
7. Does not waste words.
8. Wears clothes that are appropriate for school.
9. Compares other teachers' work with his to find his shortcomings.
10. Interprets and solves complex situations quickly.
11. Does not clear his voice frequently.
12. Studies at home in the evening.
13. Seeks more effective ways of doing work.
14. Keeps in touch with personal friends.
15. Conserves personal strength.
16. Knows where to find things.
17. Attends strictly to his own business.
18. Carries on a dinner conversation of value.
19. Discerns lack of interest and the reason for it.
20. Does what he believes to be best.
21. Reads good books.
22. Keeps skin in healthy condition.
23. Does not just sit.
24. Endeavors to correct his faults.
25. Pays bills promptly.

[21] Robinson, *op. cit.*, 214.

TABLE IV

RESULTS OF AN ATTEMPT TO IDENTIFY THE TRAIT-ACTIONS THAT WERE ALLEGED TO BE ILLUSTRATIVE OF CERTAIN TRAITS

	Trait actions																									Total number of attempts	% correct	Rank of the Charters Waples choice
Traits	1	2	3	4	5	6	7	8	9	10	11	12	13	14	15	16	17	18	19	20	21	22	23	24	25			
A	1		15		4			1		5								2	1					4		33	12½	3½
B								24						3				1				7				32	22	2
C		5	9	1					2			1	1						1		8					28	4	6
D	0	19				1	7						1			9										37	0	?
E	2		9		2						2			7					1	1						27	—	—
F		3	3						5					10					3							24	41	1
G		2				16										2	4			3					4		31	64	2½
H				5						1			3					1	0				17				27	63	1
I				2			4				9							12									27	44	1
J	21			5		1		4		1							1			3			1				37	14	2
K		1				5	3	1	1	16					2		1		8	4				1			43	2	9
L											3				21							14					38	55	1
M					1	1			3										1	15				2	8		31	—	—
N		1								1		21	3				1				1		4	1			23	3	6
O	3			14			1		1								1			2			3				25	8	4
P												3		7	1			11					2				24	13	3
Q								7								10						18					35	29	2
R			1		28				2															6			37	3	4
S									2	6			12				1	1	3	5							30	40	1
T					1				12			3	13										4	11			44	25	2
U	3					1				1							1					2		23			31	3	5
								9	0		9						4	2		1	3				3		31	0	?
V		1	4						1			8				1					21						36	58	1
W				1		9	3				4	1			5		6	1	1								32	19	2
X							7						1		3	1	1								8		21	33	2
Y																													
																										Av.	22	3

The validity of definition in terms of trait-actions. As a means of studying the validity of the Charters-Waples technique for defining traits, the following investigation was made recently by the present writers. The 25 families of traits assembled by Charters and Waples were mimeographed as shown in Table II. Twenty-five trait-actions (illustrations of these traits) were obtained from *The Commonwealth Teacher-Training Study;* these also were mimeographed (Table III). In preparing Table III the writers deliberately avoided including certain trait-actions that obviously were tautological. For example, as an illustration of the trait "optimism" Charters and Waples present the trait-action, "Looks optimistic." Useless and flagrant repetitions of this kind were of course omitted.

Sixteen faculty members of a state university and 16 graduate students from a school of education were then instructed as follows:

> Table II contains 25 so-called families of "traits." Table III contains an equal number of corresponding "trait-actions" or illustrations of these traits. On the left margin before the letter indicating each family of traits, write the number (or numbers) of the corresponding trait-action in Table III.
>
> One helpful hint will be given; namely, two of the traits in Table II are illustrated twice each, two of the traits are not illustrated at all, and the remaining 21 traits in Table II are illustrated once each in Table III.

In making this investigation the writers assumed that if the trait-actions are valid descriptions of the *traits* which they are supposed to define, faculty members and graduate students should be able to determine with a fair degree of success the particular trait illustrated by each trait-action.

The results of the study revealed that no individual was able to match as many as one third of the trait-actions with the traits. The largest number of correct responses for any one judge was 8 (32%) and for the entire group of judges, the median number of correct responses was only 5 (20%).[21]

In Table IV the letters arranged vertically at the left represent the traits given corresponding letters in Table II, and the numbers arranged horizontally across the top of the table represent the trait-actions with corresponding numbers in Table III. Table IV is to be read as follows:

1	person	thought	that	trait-action	No.	1	best	illustrated	trait	*A*.		
15	"	"	"	"	"	"	3	"	"	"	"	
4	"	"	"	"	"	"	5	"	"	"	"	
1	"	"	"	"	"	"	8	"	"	"	"	
5	"	"	"	"	"	"	10	"	"	"	"	
2	"	"	"	"	"	"	18	"	"	"	"	
1	"	"	"	"	"	"	19	"	"	"	"	
4	"	"	"	"	"	"	24	"	"	"	"	

According to Charters and Waples, trait *A* ("adaptability") is illustrated by trait-action No. 5 ("gives up preconceived notions"). Only 4 persons out of 32 (12½%) chose this trait-action correctly, but 15 agreed that trait-action No. 3 ("overlooks the deficiencies of a suburban community") best illustrates "adaptability."

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[21] Only 32 *Ss* were employed in this first investigation; additional data were later assembled, and the results set forth here were corroborated.

In Table IV the Charters-Waples choice is in bold face. At the right of Table IV the percentages of correct responses are given. For example, 12½% of the attempts made to find the trait-action that is supposed to illustrate[29] trait *A* were correct, 22% of the attempts to find the illustration of trait *B* were correct. In two instances (traits *D* and *V*), *none* of the efforts resulted in matching the trait-actions with the trait! For the entire list of 25 traits, the average percentage of correct response was only 22! Greater chaos could scarcely be imagined. Are we to conclude on the basis of these data that the "translators" who aided Charters and Waples were misguided or "incompetent"? Perhaps not. We believe that, because of overlapping of the trait-actions, the translators attempted to weigh an imponderable. Charters and Waples were aware of the fact that the trait-actions might illustrate more than one trait. But surely, if the trait-actions add to or enhance the clarity of the familiar trait name, one might expect something approaching greater unanimity in the matching.

What should a trait-action add to the trait name to bring about greater clarity and precision? One thing appears certain: The trait-action should convey an *additional,* if not a new idea, to the reader. Certainly, the trait-action should not be tautological. In some respects, a trait-action might well resemble a definition. Through expression *in action* or activity, it should portray an example of the trait at work. In a sense, therefore, a new idea should be expressed in the trait-action. Apparently, the Charters-Waples trait-actions do not satisfy these criteria.

The quantitative expression of certain personality traits. Let us examine further the problem of measuring traits. Assume for the moment that the term "intelligence" expresses a personality trait, and grant further that this personality trait is subject to rigid quantitative measurement. Degrees of intelligence may then be represented along the horizontal base-line of a normal distribution surface and the percentages of individuals displaying various amounts of intelligence may be represented vertically. Individuals who display an ordinary amount of intelligence fall near the middle of the distribution and individuals markedly intelligent or markedly unintelligent fall at one or the other extreme. He who falls near the extreme right of the curve (one highly intelligent) is considered an enviable deviate. For this and for certain other so-called traits it is valid to say "the more the better." But the foregoing statement of course does not hold for all traits.

Consider for a moment the hypothetical trait, 'emotional responsiveness.' Of this trait it is not valid to assert, "the more the better." The extreme deviate in either direction is to be pitied. The individual who falls near the average is perhaps fortunate. In other words, for certain traits, such as 'physical health,' 'mental balance,' 'intelligence,' 'reasonableness,' it is probably valid to say "the more the better;" but for certain other traits, 'emotional responsiveness,' 'self-confidence,' 'courage,' 'unselfishness,' 'cautiousness,' 'sociability,'[30] the statement does not apply. Measurement of the first type is obviously not directly comparable with measurement of the second.

It is, of course, apparent that individuals are distributed in the case of most traits according to the normal surface. And, in part, because we have found that beyond a certain point in a scale some traits are undesirable in certain environments,

[29] According to Charters and Waples.

[30] It is to be remembered of course that the very existence of these general traits as scientifically established 'quantities' has been questioned.

we have come to think of types, the introvert and the extravert, the honest and the dishonest, the clever and the dull.

This practice would perhaps be satisfactory if two conditions held: (1) the trait would have to be quantitatively measurable; and (2) the dichotomy must be thought of as a result of *quantitative dispersion,* wherein the extremes are set apart upon a base-line which expresses the full range of the distribution.

In several traits which have been somewhat clearly defined and rather reliably measured, homogeneous age groups vary according to the normal distribution surface. The so-called "opposite types" are found at the extremes of the distribution surface. But many of our trait words have been developed to convey ideas which ignore the full range of the distribution and continue to operate under the domination of the type concept. For example, of one man we remark that, "He has personality." Of another we say, "He has *no* personality." Such statements rest almost wholly on an all-or-none concept.

The foregoing concept of dichotomy probably exists when most of the trait words listed in Table II are used. It is relatively easy to find words to represent the extremes of these traits, but much harder to find terms to represent the *full distribution* of the trait. It is true that we can measure general intelligence with some degree of accuracy. We can (but perhaps seldom do) think of two types, the dull and the bright, meaning the extreme deviates. This is true also of an ability such as arithmetical comprehension. But with character traits, the story is likely to be quite different. A quantitative measure seems to be less valuable here. For traits such as courtesy and honesty (when they are not resting upon the all-or-none concept) involve an almost interminable number of reactions which make, at present, specificity or precise description seemingly impossible. In spite of the fact, however, that the trait words lack scientific accuracy, they have been appropriated by certain character testers; the patterns of behavior which they represent have been assumed to be well-restricted and somewhat precisely understood. But the words themselves have been (and continue to be) simply society's means of conveying gross, unanalyzed impressions, not of expressing rigid, accurate, and specific ideas.

Attitude words. There is still another reason for our dilemma in trait measurement. For pairs of opposites such as 'courteous' and 'discourteous' the normal curve might be employed for representing the facts. But for scientific work a two-dimensional curve does not suffice. Many words represent much more than external, objective facts. They signify certain aspects of an objective fact as well as the *attitude* or the *reaction* that is aroused. Apparently there is no available method for partialling out the relative importance of the external fact (or some aspect thereof) and the attitude of the speaker when these words are employed. This is patently true of the 'trait' words; descriptive labels which are used to convey numerous attitudes, reactions, and feelings.

Many factors combine to produce a total effect which is often described by a trait term. Among these factors the momentary attitude of the person hearing the word is a factor of no small importance.

That a trait rating represents an attitude or effect produced in addition to observance of objective behavior is not a novel thesis.[61] It is made apparent by the

[61] H. O. Rugg, Is the rating of human character practicable? *J. Educ. Psychol.,* 13, 1922, 30-42, esp. 37 f.

results that are obtained whenever trait rating is attempted. For example, one worker found that long acquaintance makes for *greater leniency* in rating.[22] This finding was doubtless due in part to the fact that with long acquaintance the individual

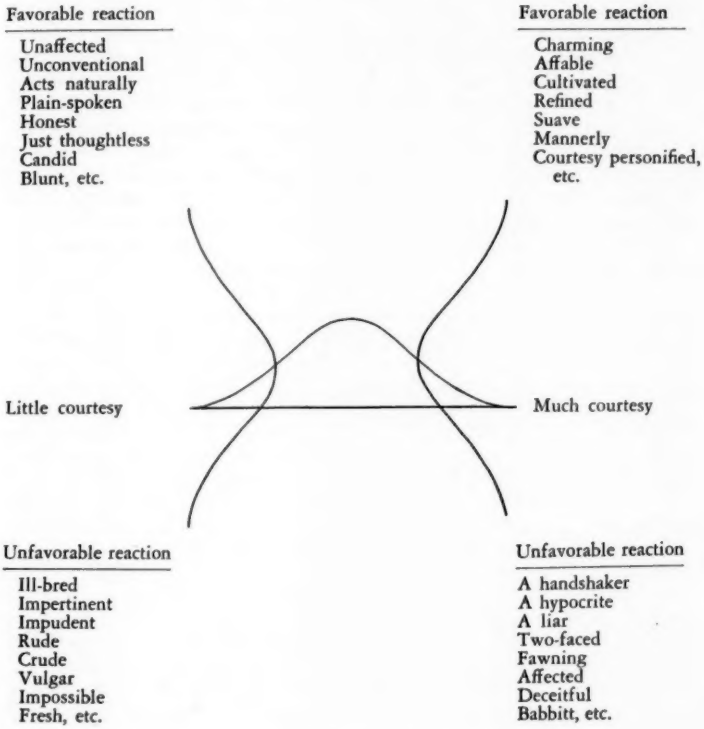

FIG. 1. VARIOUS ATTITUDES THAT MAY BE AWAKENED BY COURTEOUS BEHAVIOR

becomes inured or adapted to eccentricities, and therefore the shortcomings of one's acquaintances are frequently overlooked or disregarded. Shen reports a positive relationship between friendship and a tendency toward overestimation of desirable traits.[23] Terman, Courtis, and others have reported that ratings on school work are influenced by the halo effect that is engendered by the pupil's personal traits. In

[22] F. B. Knight, The effect of the 'acquaintance factor' upon personal judgments, *J. Educ. Psychol.*, 14, 1923, 129-142.

[23] E. Shen, The influence of friendship upon personal ratings, *J. Appl. Psychol.*, 9, 1925, 66-68.

these illustrations we find again a major difficulty which confronts us when we use trait words. Adequate or even partial control of the many variables which may affect the trait is seldom provided. Surely it is obvious that if the scientific worker would retain his critical sense of fact, he must be very cautious in using *generic* terms in experimentation.

A host of words are employed daily whose function is little more than that of expressing personal reactions or of description. Examples of such descriptive terms and their flexible use are frequent. An individual displays so-called 'thrift.' His friends say that he is 'economical' or 'thrifty.' His enemies remark that he is 'stingy' or 'penurious.' Another individual displays what some observers might call 'dignity.' Unfriendly observers accuse him of being 'proud,' 'vain,' or 'haughty.' Attitudes are of course the result of innumerable reactions; in a sufficiently large number of people, an identical situation or behavior may produce attitudes varying from extreme disgust to open idolatry. Although the excess freightage of an attitude word is not easily diagrammed, an attempt has been made in Fig. 1 to portray the implications of certain attitude words.

Fig. 1 reveals the various attitudes that may be awakened by any behavior that might be placed theoretically at some point along a distribution surface of 'courteous' behavior. By displaying extreme or unusual courtesy, an individual may arouse wholly different reactions in different witnesses. The foregoing statement holds also for the individual who is markedly lacking in what is ordinarily called 'courtesy.'

In Fig. 1 are at least 4 conceivable sets of terms which might be employed in describing one type of behavior. If the attitude aroused is friendly, one of the expressions in either of the two sets of phrases that are listed in the upper half of Fig. 1 will probably be employed to describe the person or his behavior. If, on the other hand, the observer's attitude is unfavorable or hostile, a term from the lower half of Fig. 1 (or a similar term) will probably be selected. Thus, the ratings that are attached to objective behavior are determined *by the observer* as well as by the behavior of the person eliciting the response. Possibly the incalculable nature of the *responses* that may be aroused by a given behavior is a chief reason why lists of so-called 'personality traits' are so unsatisfactory as measures of objective behavior. It is for this reason that it has been necessary to employ three curves instead of one in the construction of Fig. 1. With its three normal distribution surfaces, Fig. 1 illustrates merely the fact that two dimensions are inadequate for diagramming the total freightage of 'attitude words.' Most of the trait terms that are listed in Table II are obviously little more than attitude words.

Further remarks. The foregoing presentation gives facts which are clearly recognized by the thoughtful person. These facts, however, illustrate the difficulties encountered by those who attempt to measure traits and attitudes precisely. Certain ingenuous workers seem to have assumed the existence of 'faculties'—of static, unified, and more or less isolated characteristics which operate independently. Apparently Charters and Waples believe that verbal abstractions named traits have quantitative existence, and that they can be studied and defined with objective precision. Re-christening a 'faculty' by calling it a 'personality trait' is a matter of relatively minor importance. But the underlying concept appears essentially invalid in both instances.[24]

[24] We have still to consider whether objectivity may logically be used in personality measurement. The answer is, Yes: within reason. But the objective ap-

Dashiell has recently commented on "the fallacy of distinct professions," *e.g.* the fallacious assumption that the learned professions may be distinguished readily by psychological traits necessary in persons engaged successfully in their pursuit.[35] Dawson has gone even further. Commenting upon the Charters-Waples trait terms that are listed in Table II, Dawson remarked:

"Only one [term] 'scholarship' . . . seems to bear directly on what we have been accustomed to call teaching or even directed learning. Nearly all, if not all, of the remaining twenty-four items are desirable as traits of teachers as they are desirable as traits of all other decent and civilized people."[36]

The writers find themselves wholly in agreement with Dawson's statement for the reasons set forth in this paper. Moreover, the writers are a bit confused by Charters's enthusiastic assertion that, "the discovery of the trait is one of the most significant factors in human progress and happiness."[37] Just what is the essence of this discovery and who was the discoverer? What is the difference between a 'trait' and a 'faculty'? Is the 'trait' really a discovery, or is it merely a relapse in scientific thinking to the days of 'faculty' psychology?

SUMMARY

(1) The assumption that the so-called 'personality traits' are unitary is one that is of extremely doubtful validity. Acceptance of this assumption is, in the judgment of the writers, tantamount to an acceptance of a 'faculty' psychology.

(2) If 'personality traits' are not unitary entities, attempts to rate, to measure, and to discuss them in quantitative terms are hampered by the almost infinite numbers of reactions that are grouped under a generic trait heading. We have yet to see a character test or rating device which provides for or controls an appreciable number of the complex variables.

(3) In the light of available technique, it is doubtful that the quantitative approach to the study of personality is wholly valid. Certain traits are functions of individual reactions, of thinking, of pairs of individuals, of a situation, and of a particular concatenation of accompanying factors.

(4) Even if the so-called 'personality traits' were measurable, quantitative measures are not available for most of the traits that are listed by present-day workers. Until such measures are available, quantitative descriptions convey little precise meaning.

(5) For several reasons, the writers are convinced that the Charters-Waples attempt to define traits is not valid: (1) because the over-lapping of the trait-actions is excessive; (2) because many of the trait-actions convey no new or additional idea; and (3) because qualified students are unable to identify the traits which accompany trait-actions.

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proach must deal with adequate samples of behavior covered by descriptive terms. And objective measurement should be accompanied by or supplemented *by case-study and by subjective analyses.* We should admit frankly that, when we use most trait words, we are simply employing adjectival expression, which includes the *observation* of behavior and our *reaction* to the behavior.

[35] J. F. Dashiell, Personality traits and the different professions, *J. Appl. Psychol.*, 14, 1930, 297-301.

[36] Edgar Dawson, Review of The Commonwealth Teacher-Training Study, *The Historical Outlook*, 20, 1929, 357 ff., 358.

[37] Charters, *op. cit.*, 22.

(6) It is the belief of the writers that the Charters-Waples suggestions for the improvement of personality are far more definite than is warranted by the facts themselves.

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Northwestern University — Paul A. Witty

Dandy's Radical Extirpations of Brain Tissue in Man

The psychological literature affords very little cross-reference to the results of neuro-surgery in spite of their great theoretical importance and interest for psychology. Such data are widely scattered among surgical, neurological and clinical reports and are concerned with specific therapeutic problems. However, the facts justify an attempt to bring certain results of the neuro-surgeon into brief, though inadequate, review for psychology.

Dandy has completely removed the right cerebral hemisphere from eight patients. He has performed total extirpations of one or more lobes much oftener. The results summarized in this note are from *right-handed cases only.* We cannot undertake to examine the critical differences between them and left-handed patients.

There are tabulated below certain generalizations on the effects of removing the right hemisphere. These generalizations derive from those cases affording the longest observation.[1] The object of this note is to present only those effects which stand out as univocal and consistent in their interest for psychology.

The operation was the complete extirpation of the right frontal, temporal, parietal, and occipital lobes peripheral to the corpus striatum. The weight of the tissue removed varies, with the pathological conditions involved, from 250 to 584 grm.

Coherent conversation began within twenty-four hours after operation, and in one case on the afternoon of the same day. Later examinations showed no observable mental changes. The patients were perfectly oriented in respect of time, place, and person; their memory was unimpaired for immediate and remote events; conversation was always coherent; ability to read, write, compute, and learn new material was unaltered. Current events were followed with normal interest. There were no personality changes apparent; the patients were emotionally stable, without fears, delusions, hallucinations, expansive ideas or obsessions, and with a good sense of humor; they joked frequently. They showed a natural interest in their condition and future. They coöperated intelligently at all times throughout post-operative care and subsequent testing of function. One patient was a clergyman and a year later was conducting vesper services from his wheel-chair in the sanitarium where he lived.

Cranial nerve findings after operation were as follows:

I. Smell seemed unaffected, though a check on the pre-operative sensibility was not always possible. Various odors were promptly named.

II. Complete homonymous hemianopia for form and color.

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[1] Cf. W. E. Dandy, Physiological studies following extirpation of the right cerebral hemisphere in man, *Bull. Johns Hopkins Hospital,* 53, 1933, 31-51; also Removal of right cerebral hemisphere for certain tumors with hemiplegia, *J. Amer. Med. Asso.,* 90, 1928, 823.

III, IV, VI. All extraocular movements were normal; no nystagmus.

V. The left trigeminal domain was remarkably preserved. Light touch was lost but slightly more intense stimulation with loose cotton was recognized and located quite accurately. Sharp and dull objects were easily differentiated, though less acutely than on the right side of the face. A pin prick gave pain. Heat and cold were felt, though less acutely than on the right side. The masseter muscles contracted on both sides, but were weaker on the left. The jaw was not deflected from the midline when the mouth was open. The corneal reflexes were acute and equal.

VII. Taste was preserved in part at least; perhaps it was somewhat dulled but pre-operative data were not available. There was definite facial asymmetry with some variation from case to case: smiling drew the left side of the mouth into slight movement. The left eyelid closed tightly. The forehead wrinkled weakly on the left.

VIII. Watch tests in all cases and an audiometer record in one showed that both ears were normal. There was no dizziness.

XI. Sternomastoid and trapezius muscles functioned, but more weakly on the left side.

XII. The tongue protruded normally in the midline.

Arm, leg, and torso findings after operation were as follows:

There was a left hemiplegia; no movement was possible in the left arm. A complete flaccidity of all muscles on the left side followed the operation and was present for several hours. In none of the cases did more than a mild rigidity develop within two months. The left hand tended to assume a claw shape, though the fingers were not rigid and could be manipulated easily. One person, who lived three and a half years after operation, exhibited contracture of his arm but not his leg. In general the foot muscles moved slightly but the toes could not be moved voluntarily nor the foot lifted from the bed. Voluntary movements of the knee and thigh were not possible. There was ankle and patellar clonus. The biceps and knee reflexes were greater on the left side, though varying somewhat from case to case. Plantar stimulation caused flexion of the leg but no movement of the toes. Abdominal reflexes were equal on both sides in some cases, though decreased on the left in other cases when they had been altogether absent on the left before operation. There was complete sphincter control. Cutaneous sensibility was abolished on the left side, except in the facial areas noted above. Deep sensibility of certain kinds was present, *e.g.* quite accurately localized pain from squeezing the muscles and pain from the manipulation of joints. The patients could identify which toe was being manipulated but not the direction of the movement.

Such cases have lived three and a half years after operation in apparently normal mental condition, though Dandy is careful to point out that it is better to say that no abnormalities could be *disclosed,* since hemiplegia obviously prevented their adjustment in competition with others.

Dandy has made many less radical extirpations, removing only one or two lobes. This work yields data[3] which can be outlined as follows:

[3] W. E. Dandy, Effects of total removal of left temporal lobe in a right-handed person: localization of areas of the brain concerned with speech, *Arch. Neur. and Psychiat.*, 27, 1932, 221-224; Division of Brain Surgery in *Lewis's Practice of Surgery,* 1932.

Frontal lobes. The right or the left or both lobes may be removed with no loss of function, provided the line of excision is anterior to the prerolandic areas, the 'motor cortex,' and, in the case of the left lobe, anterior also to Broca's area.

Temporal lobes. Either the right or the left lobe may be completely removed, with the only permanent loss of function being incomplete homonymous hemianopia (right or left as the case may be). Transitory anomia does follow removal of the left lobe in right-handed persons, but disappears as the margin of the parietal lobe recovers from the trauma.

Parietal lobes. The right lobe can be removed with no loss of function provided the line of excision is posterior to the postrolandic area, the 'sensory cortex.' The left lobe, peripheral to the corpus striatum and posterior to the mesial half of the rolandic area, seems to have become localized for surgery as the most important area of the brain for mental functions. Loss of tissue from this region results in predictable and permanent loss of a variety of functions depending on the area removed.

Occipital lobes. Excision of the right lobe gives complete homonymous hemianopia and there is no other sign or symptom referable to this lobe. Removal of the left lobe produces complete homonymous hemianopia with no other loss, provided the line of excision is posterior to the supramarginal and angular gyri; if these convolutions are involved, visual aphasia results.

The *cerebellum* also can withstand the involvement of surprising amounts of tissue with no objective functional disturbance. The vermis can be incised lengthwise, the outer cap can be removed, even the cortex can be removed to a depth of 2 cm. with only temporary disturbance. However, the dentate nuclei can not be injured without devastating results.

Thus, increase in the number and amount of extirpations of brain tissue, plus the ability to carry out much operative procedure under local anaesthesia with the patient fully conscious, plus suitable unipolar electrodes for exploration, has yielded a quantity of 'experimental' data. It is of particular psychological interest that the evidence does not uphold such notions as mass action or equipotentiality with functional transfer from one region to another. On the contrary, it supports and defines specific localization.

Most of the misunderstanding on this question has arisen from faulty interpretation of data on tumors: the functions disturbed were so easily thought to be 'located' at the site of the lesion. The argument for transfer to another area arose when removal of the tumor and *immediately* contiguous brain tissue was followed by reappearance of the function. With increase of the data on radical extirpations it is seen that tumors in the surprisingly numerous silent areas exert great influence by pressure and oedema upon relatively distant and localized 'centers.' For example, the left frontal lobe has long been thought to be especially important for intellectual functions, and, while it is true that tumors so located give rise more frequently to mental effects than tumors in some other sites, this result is due to pressure and oedema of the left parietal lobe. A slightly different kind of evidence is afforded when the removal of the tissue from Broca's area to a depth of 2 cm. is followed by temporary loss of motor speech and its subsequent return; however, if the area is excised to a depth of 4 to 5 cm. motor speech never returns. Similarly, all other 'speech' functions seem to be localized in the left parietal region. Auditory and visual verbal apprehensions are separately localized; the latter, moreover, is inde-

pendent of the visual area in the calcarine fissure of the occipital lobes. The accepted teaching of bi-temporal auditory representation, inferred from tumors in the temporal lobes, now gives way to the evidence from total extirpation of either temporal lobe with no impairment of hearing. The motor and sensory localization in the paracentral region has also become more certain.

An interesting throw-back to the once popular question of the 'seat of consciousness' is offered not only by the above data (removal of the right hemisphere or the left frontal, temporal, or occipital lobe) but especially by the fact that interruption of the left anterior cerebral artery, posterior by about 2.5 cm. from the anterior tip of the corpus callosum, is fatal to the reappearance of consciousness. The patient may live weeks, but consciousness never returns nor any of the voluntary movements in any part of the body, and under these conditions reflex movements can only be elicited by very strong stimulation. Hence the deep paracentral and parietal regions of the left hemisphere, near the corpus callosum, though not posterior to the splenium, would seem to have dominant importance for consciousness. This conclusion is also born out by evidence originating below this region, since pressure on the midbrain, pons and sometimes the medulla induces drowsiness or unconsciousness.

At the present stage of knowledge we can summarize the *critical* cerebral zones for the right-handed person as being the paracentral region including Broca's area, the left parietal lobe (most important for mental activity) back to and including the supramarginal and angular gyri, and the calcarine fissure in the occipital lobes.

Harvard University KENNETH DIVEN

OPINING

Obviously related to belief stands a group of topics which includes supposition, conjecture, doubt, query, conviction, and opinion. When these words appear in this form they suggest human inclinations toward questions of fact or truth. But if we say "believing," "supposing," "conjecturing," and so on, we suggest an active functioning of the organism and therewith an interesting and important group of psychological problems. Upon this entire group of operations psychologists have done very little work of a descriptive, not to say of an experimental, kind.[1] In his approach to the topics of opinion and opining from the field of public speech, the writer has found but little aid in psychological books and articles. He sets down, therefore, a preliminary sketch of a functional treatment of the subject in its more active form of opining.

Psychologists have agreed upon no clear and standard definition for 'opinion' and the more active 'opining.' If we accept for the moment the common definitions of the dictionaries, we may regard opinion, or *an opinion,* as a belief or persuasion held with or without assurance by an individual, but not generally agreed upon by the group or community. So far as the social sanction is concerned, therefore, only the individual is committed to the validity or truth of the opinion. However tenacious the individual may be in asserting and standing for his view, and however

[1] A few exceptional titles may be noted. W. L. Gard, A preliminary study of the psychology of reasoning, this JOURNAL, 18, 1907, 490-504; T. Okabe, An experimental study of belief, *ibid.,* 21, 1910, 563-596; M. E. Bulbrook, An experimental inquiry into the existence and nature of 'insight,' *ibid.,* 44, 1932, 409-453.

strongly he may be convinced of its validity, the sanction tends to be only an individual sanction. Where opinion is described as "stronger than impression, less strong than positive knowledge" (Webster's New International Dictionary) the definer evidently has in view objective or social acceptance rather than the conviction of the opiner. We commonly reserve the unpleasant epithet "opinionated" for unwarranted conviction and sturdy assertion. The verb "to opine," however, implies a certain modesty and reserve in expression, as if the opiner knew the supposititious and insecure nature of his views. In our further comments upon opining we shall assume this connotation.

We give a few hypothetical instances of this active process.

(1) Brown, a prominent citizen, is urged to attend a mass meeting on the local problem of taxation and to advise the community. The problem is new to him but he proceeds systematically with the task, formulates the problem, analyses it, and collects, arranges, and weighs evidence and argument. As he proceeds, several possible solutions of the problem occur to him. He examines each with care, weighing its advantages and disadvantages, and attempting to foresee the consequences of its adoption. Eventually he arrives at a conclusion regarding what should be done. He has formed "an opinion." He opines a certain solution.

This is an instance of thinking in the stricter sense of that term, "a search initiated by a problem or task and carried on by the aid of symbolic objects."[2] Under occasional instruction Brown formulates a problem, manipulates symbols (chiefly verbal) and finally arrives at a tentative solution.

Several aspects of the process deserve special notice. It should be observed, in the first place, that while it is primarily the function of thinking that we here see in operation, other psychological functions are also involved. In reading the Sunday paper Brown's eye is caught by an article on taxation, and he remarks, "Might throw light on our problem" (inspection and comment); he digests the contents of the article (comprehension); he reaches for his pencil to note down a set of statistics (action); he recalls his experience in dealing with a similar problem in the past (remembering) and he imagines the practical consequences of this solution and that. Indeed, it is doubtful whether there is any one of the basal psychological functions that might not be involved at some stage in the process.

One practical consequence of the manipulation of symbols during the second stage of the process is that one or more solutions become identified with, or incorporated in, existing action determinations. Brown may generally act in ways that will decrease his annual expenditure. As he examines the argument, it becomes clear that such and such a course of conduct would reduce his taxes. The result is that the course of conduct becomes incorporated in the general inclination to do whatever will decrease annual expenditure. It would be tedious and futile to attempt an enumeration of all the inclinations of the organism which might be involved in his thinking. But it is safe to say that the process might involve the individual's habits, concrete past experiences, dominant interests, and general relation to the social environment. It is probable also that current functional employment and inner organic states might be involved; fatigue, for instance, might lead Brown to accept a plausible solution without careful consideration of its consequences.

(2) In glancing through his favorite magazine, Riker comes upon an advertisement of a new safety-razor. He reads the persuasive statements which dwell upon the

[2] M. Bentley, *The New Field of Psychology*, 1933, 89.

alleged advantages of the razor, finds them convincing, and ends by mailing the coupon at the bottom of the page. He has formed at least a tentative opinion favorable to the new razor.

The first function involved here is that of comprehension. Riker reads and understands. The argument is a manipulation of verbal symbols in the direction of buying articles that are cheap and conducive to personal comfort. Comprehension of the topic constitutes a self-instruction, and the coupon at the bottom of the page constitutes a formal instruction leading to a determination which issues in the appropriate act of signing his name.

(3) Morgan reads in the news that a local fire has made several families homeless. He considers the desirability of raising a subscription, a customary response to a situation of this kind. He proceeds at once with his plans. He forms an opinion immediately without going through the processes of thought and without being influenced by argument or suggestion from others.

Morgan's reading involves comprehension. As he reads he probably also imagines scenes depicted and perhaps recalls similar scenes witnessed in the past. In this case the manipulation of symbols, either by himself or others, is unnecessary. There already exists in the organism an habituated trend toward the course of conduct followed, and the opinion follows immediately upon his comprehension of the need.

(4) While at the club Downing hears a neighbor declare that another world-war is inevitable. He has never given the matter thought, and now hears no argument in support of the view. But he has a very high opinion of his convinced neighbor and accepts his views uncritically. Consequently he immediately adopts as his own the gloomy opinion.

Here the expression of opinion, when comprehended by Downing, results immediately in an adoption of it. An important factor is his readiness to accept his neighbor's views.

(5) A business man hears that the men of his set support the NRA. He has not given thought to the matter, but after sounding his associates on the topic he immediately accepts what appears to be the prevailing view.

In this case the discovery that members of his group are supporting the NRA (comprehension) results in immediate adoption of an opinion. The process is facilitated by a fixed inclination to adopt the opinions and to imitate the behavior of other members of his social group.

Such a cursory examination of hypothetical instances suggests that opining can be resolved into functions already familiar to the psychologist. All five instances examined involve comprehension; most of them involve one or more of the apprehending functions; one instance clearly involves inspecting and thinking. Whether opining exhibits a characteristic functional pattern that would distinguish it psychologically from such procedures as supposing, conjecturing, and the like, could be determined only by taking the problem into the laboratory.

It seems reasonable to suppose that a satisfactory analytical description of the active opining might be secured by report under experimental conditions. Let us suppose that the subject is presented typed or oral proposals to which the verbal report "yes," "no," or "doubtful" is applicable and of such a nature that *S* would seldom be either completely in doubt or perfectly assured as to the appropriate answer. The questions should not admit of ready-to-hand answers, and they should

represent a wide variety of situations in which one naturally opines. Besides the formal report, the kind and degree of assurance should be set down together with whatever grounds of assurance were at hand.

Since the process of opining obviously rests upon performances of the living organism which may fairly be called psychological, it would seem to be advisable to translate this process into these terms. In clearing the way for this translation, we have proposed a number of fair, though hypothetical, instances of opining, which seems to suggest that this activity is neither unique or *sui generis* but rather involves a number of recognized functions combined in characteristic ways and leading on to characteristic products which may well be subsumed under the general term "opinions."

Cornell University WILLIAM E. UTTERBACK

McCOSH ON THE EMOTIONS

It is an historical truism that discoveries and theoretical interpretations in all of the sciences have shown many anticipatory symptoms. Credit is, therefore, difficult to give in any one particular case. Usually the man who crystallizes the doctrine and presents it in such a way as to attract the public imagination receives the highest tribute. By the same token it is not easy to establish a date for the first appearance of many scientific discoveries and interpretations. Although for centuries there were approximations to the theory of evolution, Darwin deservedly caught the public ear in 1859 with his *Origin of the Species by Means of Natural Selection*. The same thing may be said for the almost simultaneous discovery of the mosaic pattern for the distribution of cutaneous qualities on the skin by Blix, Goldscheider, and Donaldson just prior to 1885. In more recent times, Köhler, Koffka, and Wertheimer have received credit for the more precise formulation of a Gestalt theory which was many decades in the making.

No theory of the emotions has received wider and more prolonged attention than the James-Lange-Sergi theory, first advanced by James in 1884, but also announced independently by Lange in 1885 and by Sergi in 1894. Titchener has made the psychological world his debtor by pointing out in an extended scholarly article how this theory was anticipated by almost a score of writers over a period of about two and a half centuries.[1] He is, therefore, puzzled by the fact that Lange specifically referred to earlier writers who seemed to have anticipated his theory, while James, who was a wide reader, quoted only Henle and then with the remark: "Note how justly this expresses our theory!" Henle's definition of emotion, which occurs on the same page and is not unlike the James formula, is, however, not cited.

In gathering materials for a treatise on the emotions I have quite accidentally come upon another approximation to the James theory in a book which must have been known to James at the time. The book is called *The Emotions* and its author is James McCosh, at the time president of Princeton College. It was published by Charles Scribner's Sons in 1880 but is now out of print. I am really amazed to find so far absolutely no references to it in the hundreds of references that I have gone over. It was inadvertently discovered by me through an advertise-

[1] E. B. Titchener, A historical note on the James-Lange theory of emotion, this JOURNAL, 25, 1914, 427-447.

ment in the back of Ribot's *German Psychology of Today* published by the same house. Curiously enough James mentions McCosh in his index, referring to p. 501 in the first volume of the *Principles,* but this is a typographical error which should read p. 591. The citation is, however, to another book by McCosh, *Psychology, the Cognitive Powers,* 1886, and alludes to his discussion of the 'law of similarity' in connection with the topic of Association. In this otherwise remarkable book on *The Emotions* occurs a very striking passage. McCosh first discussed at length and with numerous citations the expression of the emotions in a variety of ways throughout the body. There is every indication that the author explicitly understood the literature and even advanced a step or two beyond it. For example in a previous section he wrote:

> "I have often remarked," says Burke, "that on mimicking the looks and gestures of angry or placid, or frightened or daring men, I have involuntarily found my mind turned to that passion whose appearance I endeavored to imitate." Here is an important fact, but it is not correctly stated; that which comes first is put last. The only effective way of mimicking a passion is to call up by the fancy an object or scene fitted to awaken the feeling.
>
> I rather think that sympathetic action is to be accounted for very much in this way: we put ourselves in the position of others, by calling up by the idea the same feelings, which go out in the same manifestations. Tears shed are apt to call forth tears in the beholder, or quite as readily in the listener to the tale told which makes us realize the position. It is the same with laughter, which is apt to be echoed back till the noise rings throughout a large assembly. When a company as a whole is moved it is difficult for any person to keep his composure.[2]

He made mention of what he called 'sympathetic imitation' or 'sympathetic action.' Now comes the section which is as clear a statement of the Jamesonian position as I have seen outside of James:

> We are here in the heart of a subject which cannot be cleared up at present —the reaction of mind and body. If it be true that emotion produces a certain bodily state, it is also true that some bodily states tend to produce the corresponding feelings. Dr. Braid, in his very curious experiments as to hypnotism, found that a person put in the attitude of devotion became devout. I am not disposed to speak dogmatically about this mysterious phenomenon, but I believe that association of ideas has to do with it. The act of kneeling will naturally suggest the feelings we cherished when we knelt. If we take the attitude of striking, the idea of fighting will be suggested. If the expression of affection, or of pity, is assumed, it will call up the feeling associated with it. In the very act of bringing a cloud on the brow the idea of care will be brought up.[3]

Obviously while the reversed process of bodily activity produces an emotional effect of a psychical nature it is not an exclusive process but an alternate possibility and, while the theory is consequently not so definitely worked out in a one-sided manner as in the case of James, it is, nevertheless, a very definite statement of that contingency.

It is hardly unlikely that this book was unknown at the time, since McCosh must have been a prominent scholar of the day. Dr. James McCosh (1811-1894) was a Scottish-American metaphysician and Presbyterian clergyman who became president of the College of New Jersey in 1868 and resigned the presidency in 1888.

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[2] J. McCosh, *The Emotions,* 1880, 102-103.
[3] *Ibid.,* 105.

By that time the institution had become known as Princeton College. Warren groups James McCosh with Thomas Reid and Dugald Stewart as members of the school of Scottish intuitionism. They regarded mind "as a collection of faculties, each fully capable of performing its function from the beginning. . . . But while their psychology is thus furnished with *a priori* elements for knowledge, the actual rise of knowledge is regarded as strictly empirical."[4] Cattell in his dedicatory address on the occasion of the inauguration of the Psychological Laboratory at Wittenberg College, specifically mentioned him in connection with the history of American Psychology. He said, "For earlier work tribute should in passing be paid to James McCosh, Presbyterian clergyman from Scotland and president of a Presbyterian college, who at Princeton promoted the study of organic evolution and physiological psychology. George Trumball Ladd, also a clergyman, was called to Yale in 1881 and developed there courses in physiological psychology, leading to the publication in 1887 of his *Outlines of Physiological Psychology*."[5]

Boring refers to him as "one of the last of the old school of philosopher-psychologists who therefore finds no other mention in this book."[6] Pillsbury heads a section in his history of psychology with McCosh's name—indeed the section just before that on James!—and says, "One of the men who deserve more than passing mention is James McCosh, who came to Princeton from Belfast in 1868 when disappointed in his efforts to receive the appointment at Aberdeen won by Bain. He was for long time professor of philosophy and president of Princeton University. An adherent in the strictest form of the Scotch School, he wrote volumes on *The Cognitive Powers* and on *The Motive Powers* that had a psychological bearing, but gave no great evidence of originality."[7] The reader will note the omission of *The Emotions* from this list. Murphy does not mention him in his *Historical Introduction to Modern Psychology*, but Baldwin, a student of his, lists him among the philosophers with two columns of books and articles to his credit in the *Dictionary of Philosophy and Psychology*.

A man in this position, writing a rather lengthy and significant book on the emotions in the year 1880, could hardly have been overlooked by a fellow psychologist at Harvard. That he was known to James goes without saying in as much as his name occurs in the *Principles of Psychology*. It is surprising, too, that Titchener's historical review fails to mention this contribution to the James-Lange doctrine.

University of Iowa CHRISTIAN A. RUCKMICK

BOOK REVIEWS IN PSYCHOLOGICAL PERIODICALS

Book reviews in general literature obviously serve as a means of revealing content, indicating merit, and orienting the new publication in its special field as well as in its broader setting in the world of literature. This seemingly humble chore often becomes the recognized contribution to literature and literary criticism.

In psychological journals many book reviews are worthy of distinction not only

[4] H. C. Warren, *A History of Association Psychology*, 1921, 12.
[5] J. McK. Cattell, Early psychological laboratories, *Feelings and Emotions: The Wittenberg Symposium* (ed. by M. L. Reymert), 1928, 432.
[6] E. G. Boring, *A History of Experimental Psychology*, 1929, 517.
[7] W. B. Pillsbury, *The History of Psychology*, 1929, 235-236.

because of literary quality but more so because of a special contribution to psychological literature. This paper is limited, however, to a study of several quantitative aspects concerning book reviews that appeared in journals listed in the directory of American Psychological periodicals during 1933.

The results of this inquiry are shown in the tables that follow. There were 305 reviews on 254 different books, covering approximately 419.5 pages. Of the books printed in the English Language, 53% were not reviewed until two years or more after publication. The same trend but with more delay is revealed for books written in a foreign language.

TABLE I

NUMBER OF PAGES AND NUMBER OF REVIEWS IN 1933

Periodical	Approximate total number of pages	%	Number of reviews	%
Jour. of Abnormal and Social Psychol.	64.75	15.4	40	13.1
American Journal of Psychology	107.00	25.5	133	43.6
Journal of Applied Psychology	20.25	4.8	14	4.6
Psychological Bulletin	109.50	26.1	67	22.0
*Psychological Clinic	11.50	2.7	7	2.3
Psychoanalytic Review	40.50	9.7	28	9.2
Journal of Social Psychology	9.75	2.3	4	1.3
Journal of General Psychology	33.00	7.9	8	2.6
Pedagogical Seminary	23.25	5.6	4	1.3
Total	419.50	100.0	305	100.0

* Last quarterly issue not available.

TABLE II

DATE OF REVIEW COMPARED WITH YEAR OF PUBLICATION OF BOOK

Date of review	English language (219 books)	Foreign language (35 books)
During year of publication	8.2%	0.0%
1 yr. after publication	36.1%	17.1%
2 yr. after publication	36.1%	22.9%
3 yr. after publication	15.1%	37.1%
4 yr. after publication	1.8%	5.7%
6 yr. after publication	0.0%	5.7%
7 yr. after publication	0.0%	2.9%
10 yr. after publication	0.0%	2.9%
12 yr. after publication	0.4%	5.7%
Not indicated	2.3%	0.0%
Total	100.0	100.0

Most books are reviewed only once. It is interesting to note that "The Physical Dimension of Consciousness," E. G. Boring, was reviewed four times and "Habits: Their Making and Unmaking," K. Dunlap, was reviewed six times. There are not many assiduous reviewers. About a maximum of one book review per month during the year is confined to only two reviewers.

TABLE III

NUMBER OF TIMES SAME BOOK IS REVIEWED

No. Times Reviewed	No. Books	%
1	218	85.8
2	25	9.8
3	9	3.6
4	1	.4
6	1	.4
Total	254	100.0

According to Table III, constant readers of only one journal may very likely miss many book reviews of particular interest. The Psychological Abstracts more or less serves to acquaint the reader with many of these books that have not appeared in reviews. But a more important fact revealed by this table is that there is a need for a critical evaluation of books by more than one reviewer.

TABLE IV

NUMBER OF BOOKS REVIEWED BY EACH REVIEWER

Number reviews	Number reviewers	%
1	95	60.9
2	30	19.2
3	16	10.3
4	4	2.6
5	5	3.2
6 to 10	4	2.6
10 plus	2	1.2
Total	156	100.0

TABLE V

STATUS OF REVIEWERS WITH RESPECT TO THE AMERICAN PSYCHOLOGICAL ASSOCIATION

	Number of reviews	%
Associates	102	33.5
Members	147	48.2
Non-members	48	15.7
Not identified	8	2.6
Total	305	100.0

This study shows in general that the available facilities for book reviews are not being used to the fullest extent. A number of obvious defects appear in the present system of book reviewing. These are briefly as follows:

(*1*) A small number of pages are given to book reviews in all but two journals; 51.6% of the space utilized for book reviews is confined to THE AMERICAN JOURNAL OF PSYCHOLOGY and the *Psychological Bulletin.*

(*2*) Most of the journals review few books. THE AMERICAN JOURNAL OF PSY-

CHOLOGY reviewed the largest number of books in 1933, amounting to 43.6% of the total.

(3) There is considerable delay in publication of reviews. Only 8.2% of the books published in the English language are reviewed during the year of publication and 46.6% of the reviews do not appear until two years after publication.

(4) Of the books reviewed 85.8% were reviewed but once.

(5) There are not many assiduous reviewers; 60.9% of them were credited with only one book.

(6) There is a question as to the professional competency of the reviewers. Only 42.2% of the books were reviewed by full members of the American Psychological Association, and over 15% by non-members.

The Psychological Corporation
New York, N.Y.

RICHARD S. SCHULTZ
HELEN PALLISTER

THE NORWEGIAN PSYCHOLOGICAL ASSOCIATION

The Norwegian Psychological Association was organized on April 4, 1934. The aims and objects of the Association are similar to those of the American Psychological Association.

The following officers were elected: Dr. Harold Schjelderup, President; Dr. Richard Eriksen, Vice President; Aase Gruda Skard, Secretary. The Association may be addressed care of Psychological Institute, University of Oslo, Norway.

Mooseheart Laboratory of Child Research

MARTIN L. REYMERT

THE CAMBRIDGE MEETING OF THE SOCIETY OF EXPERIMENTAL PSYCHOLOGISTS

The 30th annual meeting of experimental psychologists was held at Harvard University on April 3–4, 1934 under the chairmanship of Professor E. G. Boring. The meetings were attended by 18 members from 11 laboratories. After a short business session, reports of work in progress were received from the following universities and colleges: Brown, Clark, Columbia, Cornell, Harvard, Illinois, Pennsylvania, Princeton, Smith, Vassar, and Yale. Professors K. Lewin and M. Wertheimer were guests of the Society.

The next annual meeting will be held at Yale University on Thursday and Friday April 4–5, 1935, under the chairmanship of Professor W. R. Miles.

S. W. F.

MEMORY OF RACCOONS

In the spring of 1931 we conducted several experiments on raccoons in order to test their learning ability. We motivated them by depriving them of all food for a few days previous to the experiments. We then induced them to attempt to get out of a puzzle box with a lift latch by putting food outside of the box in sight of the animals. The first raccoon got out of the box the first time in 17 min., the second time in 4 min., the third time in 50 sec., the fourth time in 15 sec., and the fifth, sixth, and seventh times in 2 sec. each. We then considered the learning experiment completed. Two other raccoons showed similar learning curves.

A year later (1932) we repeated the experiments using the same raccoons as

subjects, the same puzzle box, and the same hunger motive. In all three cases the raccoons got out of the puzzle box immediately, within 2 or 3 sec. We then repeated these experiments one year later (1933) with the same results, the raccoons getting out of the puzzle box immediately, within 2 or 3 sec. These results thus obtained under carefully controlled and carefully timed and carefully repeated conditions seem to point to the remarkable memory of raccoons for trial and error learning such as was represented in these experiments.

Mount Union College A. B. KITZMILLER

THE TWENTY-NINTH ANNUAL MEETING OF THE SOUTHERN SOCIETY FOR PHILOSOPHY AND PSYCHOLOGY

The twenty-ninth annual meeting of the Southern Society for Philosophy and Psychology was held in the Tutwiler Hotel, Birmingham, Alabama, March 30 and 31, 1934, under arrangements made by the Birmingham-Southern College.

All sessions but that on Saturday morning were divided into (A) Psychology, and (B) Philosophy. In the Friday morning Psychology session, with Joseph Peterson in the chair, the following papers were presented: A Method for the Investigation of Palmesthetic Sensitivity, by Beverly von Haller Gilmer (University of Virginia); The Diminished Seventh, Excitement, and Musical Relativity, by C. P. Heinlein (Florida State College for Women); An Example of the Apparent "Persistency" in the Auditory Mechanism, by R. C. Wingfield (University of Virginia); A Case of Anomalous Localization in a Skin Graft, by Lyle H. Lanier (Vanderbilt University); Visual Factors Influencing the Duration of Post-Rotational Nystagmus, by O. H. Mowrer (Princeton University); Flicker Relations within the Fovea, by Frank A. Geldard (University of Virginia); and Timing Devices in the Psychology Laboratory, by Knight Dunlap (Johns Hopkins University). The philosophers' program, under the chairmanship of Anna Forbes Liddell, was The Great Analogy in Idealism, by L. E. Loemker (Emory University); Logic and Functionalism, by P. A. Carmichael (Converse College); The Constancy of Human Values, by W. Preston Warren (Furman University); and The German Church and Liberalism, by Fritz Marti (Hollins College).

In the afternoon, H. M. Johnson being in the chair, Wayne Dennis (University of Virginia) made a preliminary report on An Experimental Study of Human Instincts. This was followed by a report on The Maturation and Learning of Serial Time and Muscular Coördination Involving Visual Stimuli, by Allen O. Gamble (Birmingham-Southern College); and one on Equivalent Stimulation During Learning, by Dael L. Wolfle (University of Mississippi). Due to the absence of certain members the program of papers was short, allowing more time for discussion. Four papers by the philosophers, under the chairmanship of Albert G. A. Balz, were presented and discussed: Measurement in Plato's Republic, by L. M. Hammond (University of Virginia); The Metaphysical Postulates of Mediaeval Aesthetics, by Katherine Gilbert (Duke University); A Basis for Modern Ethics, by Anna Forbes Liddell (Florida State College for Women); and Ethical Relativity, by Marjorie S. Harris (Randolph-Macon Woman's College).

The presidential address given after the banquet, Friday evening, by the President, Roy M. Dorcus, dealt with psychological research in colleges.

The papers presented in the joint session Saturday morning, presided over by

President Roy M. Dorcus, were The Undergraduate Curriculum in Psychology, by P. F. Finner (Florida State College for Women), chairman of the Committee on the Teaching of Psychology; Thought as Awareness and Thought as Behavior, by Marten ten Hoor (Tulane University); Modern Nerve-Physiology and Descartes' Doctrine of the Production of Bodily Movement, by H. M. Johnson (American University); St. Thomas and the Establishment of Modern Science, by Albert G. A. Balz (University of Virginia); The Problem of Psychic Reality, by H. C. Sanborn (Vanderbilt University); and The Secret of Aristotle, by George G. Leckie (University of Virginia).

In the final session of the psychologists' section on Saturday afternoon, under the chairmanship of J. E. Winter, H. W. Martin (Emory University) led out with a paper on The Development of Judgments of Traits through Practice and Correction. The other papers were The Selection of Students for Mental Nursing, by G. Wilson Shaffer (University of Baltimore); The Effects of Hypnotically Suggested Blindness, by Frank A. Pattie, Jr. (Rice Institute); The Relation between Pitch Discrimination and Accent in Modern Languages, by Emily S. Dexter (Agnes Scott College); Measuring Attitudes towards the Specific Social Rights of the Negro, by Euri Belle Bolton (Georgia State College for Women); The Inter-Relations of Certain Physiological Measurements and Aspects of Personality, by Katherine T. Omwake (Agnes Scott College); and The Use of the Drake Musical Memory Test for Awarding Scholarships, by Raleigh M. Drake (Wesleyan College). The philosophers devoted their final session to a round-table discussion of The Teaching of Philosophy in Colleges, under the chairmanship of Herbert C. Sanborn who is chairman of the Committee on the Teaching of Philosophy.

At the business meeting, following the Saturday morning session, the Committee on the Teaching of Philosophy was continued and was authorized "to prepare a digest and summary of the data collected, together with such recommendations as it may deem desirable, and, with the approval of the Council, that such a report be transmitted to the institutions concerned." After a discussion of the report submitted in mimeographed form by Professor P. F. Finner, Chairman of the Committee on the Teaching of Psychology, it was voted that the report of the chairman "be placed on file subject to the approval of the Committee as a whole, and that further release of the report to outside institutions or individuals be withheld until such release is approved by the Council." Certain members of the Committee had not seen the revised report since it was mimeographed. The officers elected were John E. Winter, University of West Virginia, President, and Albert G. A. Balz and Frank A. Pattie, Jr., members of the Council. The Society voted to accept the joint invitation by Vanderbilt University and George Peabody College for Teachers to meet next year in Nashville, Tennessee. The time of the meeting was set for April 19 and 20, 1935.

George Peabody College JOSEPH PETERSON

BOOK REVIEWS

Edited by JOSEPH PETERSON, Peabody College

The Blind in School and Society: A Psychological Study. By THOMAS D. CUTSFORTH. New York, D. Appleton & Co., 1933. Pp. xix, 263.

"The purpose of this book is to acquaint the seeing with the blind and the blind with themselves." To accomplish this end the author presents a series of studies bearing upon various phases of the social psychology of blindness. He begins with two chapters designed to show how lack of vision affects early mental development and gives a striking case of retardation which he explains in terms of a restricted home environment resulting from mistaken ideas of a blind child's needs and possibilities. He finds two major difficulties in the social education of the blind, the danger of verbalism as a substitute for concrete experience and the tendency to egocentricity as a compensation for thwarting. These themes recur frequently throughout the book. This tendency to verbalism is constantly fostered in the literary type of education given the blind. "Never yet has the writer seen an object lesson taught in which there has not been an evident attempt on the teacher's part to read into the experience some sensory perceptions that do not exist for the pupil and at the same time utterly to neglect and discredit whatever reality may exist in the experience of the pupil" (p. 60).

Chapter IV, on the phantasy life of the blind, is a distinct contribution to analytic literature. The author's point of view is the phantasy life of the blind does not arise from their primary physical disability, but from the social relations that such a disability involves. Curtailment of self-expression results from the blind's living in the world of the seeing and is registered emotionally. Thus, the act of asking a stranger the name of an approaching street car is an admission of inferiority for which there must be compensation. Moreover, with loss of vision, communication depends largely on hearing and an uncanny perception of hidden meanings develops. Voices convey not only the meanings of the spoken word, but also those wordless forms of language which are made evident to the seeing by facial expression, gestures of hand, head, and body. Since social convention demands a greater control of visible expression than of voice modulation, the blind are keenly aware of their inability to grasp many subtle indications of meanings of the seeing. But the blind develop a keenness for the perception in the voice of the seeing of "a significance as clear and unmistakable as the eye perceives in the gesture of tapping the forehead." Emotional disturbances and compensations naturally arise from detections of duplicity. The seeing persons who practice duplicity "have learned to lie to the blind with words only. Their duplicity is only partially complete. They have not mastered their intonations and modulations as artfully as they have chosen their words. The phantasy life of the blind reveals that many violent phantasies arise largely from this unfortunate situation" (p. 104). Thus the environmental situation of the blind child compels him to become self-centered and this frequently results, according to the author, in speech and personality defects. The chapter continues with an extended discussion of specific causes for speech defect both physical and mental.

Chapter VI is devoted to various problems in the emotional life of the blind and gives the seeing an insight into the social attitudes of the blind which could come only from a thoughtful and keenly observant blind author. Chapter VII on the sex behavior of the blind covers the ordinary topics, but shows their unusual importance in view of the fact that a large percentage of the blind are segregated in residential schools. "Since it is impossible in most cases for the blind school child to talk, walk and work with the opposite sex, his social development takes place in the realm of phantasy, where there are no social codes or taboos" (p. 151). Chapter VIII on the aesthetic life of the blind is a closely reasoned study of the changes in meaningful wholes when the important substitution of touch for vision is overlaid by the personality changes incident to blindness.

From the beginning of the book pedagogical factors receive attention and frequent suggestions are given for a wiser treatment of blind children in the home, in the school, and in society. The pedagogical point of view becomes paramount in Chapter IX, where institutions for the blind are severely criticized. "The creative spirit in the education of the blind in America died with Samuel Gridley Howe in 1872. The educators since that time have satisfied themselves by imitating the material achievements of that pioneer, but none has dared to go beyond what he was able to achieve." They have imitated his material results solely by building bigger and better institutions, with bigger and better libraries; by devising better and more efficient punctiform systems of reading and mechanical devices for printing; and by making improvements in shops, shop methods, and school equipment; but "they have encumbered the entire system of education with stifling, sacrosanct concepts which are the traditional mores of institutional life throughout the country" (p. 199). In such an environment individuality and personality have little chance for development; it merely equips pupils with the concepts and social behavior appropriate to those who are to be social and economic dependents. The seeing world requires those weak and self-effacing attitudes which it at the same time despises in the blind. "First of all, they should live according to the way of God, which happens to be the way of each particular institution. They should practice the Christian virtues, especially stressing obedience, respect for superiors, turning the other cheek, more blessed to give than to receive, honesty is the best policy, early to bed, and it pays to advertise. They must always appear cheerful and pleasant to others and on all occasions should appear happy" (p. 207). The final chapter is devoted to striking case studies of two blind boys in a college community.

Cutforth's book is an important contribution to our literature upon the blind. Although appearing under the guise of science it is written with the zeal of the reformer and the extravagance of the iconoclast. Pedantic with *Gestalt* and psychoanalytic terminology—with a glossary for the uninformed lay-reader—and dogmatic with the implicit assumption that a blind man must know the truth about the blind (though it is not always clear how a person not born blind should presume to speak for this small and highly specialized group) the author has produced a book that will be widely read and hotly discussed. His blind readers take violent exception to his statement "under the conditions imposed upon them by society, not one of these sixty thousand human beings has developed, or can possibly develop, a strictly normal personality" (p. 2), and resent many generalizations which do not accord with their own experience. Seeing persons working for the blind wonder whether the book

will do more harm than good to their cause; they question whether the general public is prepared for such frank statements on many questions and fear that the violence of his strictures may alienate the very people in institutions who might profit by a consideration of the issues he raises. There is no doubt, however, that the author has written many true statements, some based upon experimentation and some rising from keen observation and thoughtful consideration.

Mt. Holyoke College SAMUEL P. HAYES

Behind the Door of Delusion. By INMATE WARD 8. New York, Macmillan Co., 1932. Pp. xiii, 325.

This book, describing an insane hospital, is written in the fluent, witty style of a professional newspaper man. Though lacking the dramatic intensity of Clifford Beer's great book, it is characterized by reportorial conscientiousness, avoidance of sensationalism, and an effort to be fair to everyone, including those in authority. It paints a haunting picture of the dreary monotony of the well-regulated institution. Unlike most insane writers, the author is fairly objective. Without attempting to deny the mental malady (dipsomania) which led him to consent to commitment, and without concealing the other traits which make him a sorry character, he nevertheless does not intrude himself very much upon the reader. He is more concerned in portraying the environment, his fellow inmates, and some of the attendants and doctors he has known both in the institution and outside of it. As might be expected, nearly every chapter contains examples of the author's fallacious reasoning, warped values, and questionable ethical outlook; but after all, we do not go to the insane for our logic and our ethics, nor does the author expect us to. He merely contends, perhaps rightly, that as one of the insane, living the life they live, "the recipient of their confidences," seeing them "when they are not on guard," he knows the fellow-inmates of his ward better than the physicians and attendants who "must always look down on the mentally afflicted from a height." He implies that the very position of exercising disciplinary authority necessarily precludes a profound and thorough acquaintance with wary ones under this authority. This patient has spent his entire time so far in the receiving ward, and every man admitted to the institution has therefore come under his trained reporter's eye.

Delusions of grandeur, delusions of persecution, manic-depressive states, religious manias, epilepsy, post-encephalitic conditions, dementias, and other familiar types are portrayed in brief vignettes in non-technical language; but it is the author's comments on "the sane insane" that are most interesting. He divides them into two classes, the criminals who feign insanity to escape imprisonment and the helpless dependents who are put away by heartless relatives. In some places, it requires only the signature of two or more doctors to procure a commitment; and once committed, a man rarely secures his release unless someone outside the institution will act in his behalf. He can not send out mail or even secure writing materials, if the authorities forbid it. He can not see visitors without official consent. He can not even make a telephone call. The author tells an amusing tale of a young farmer whose wife telephoned the county officials that her husband had suddenly gone insane. He was brought before a county lunacy board composed of three general practitioners. On the wife's testimony, the board committed him to the asylum and he was locked up before nightfall. The next day, the wife was

committed on the complaint of several neighbors. "A thorough examination brought out the fact that those physicians had committed a sane man on the complaint of an insane wife, and they had both man and woman before them at the husband's hearing." The hospital authorities soon released this man; but the author is convinced that there are many aged people, paralytics, and sick people who are not really insane but are confined for life against their wills because relatives do not want them and they are incapable of self-support. In another chapter, entitled "The Sterilization Spectre," the author reminds his readers that according to the records of Salem witchcraft "among the witnesses in several cases were a 'leech and a minister.' " His arguments against sterilization are of course inconclusive when weighed against the well-known arguments in favor of it; but he shows how some of the insane feel.

Though the author pokes fun at "Dr. Pomposity Nincompoop" and his ilk, he has no grudge against doctors in general, and his attitude toward the hospital attendants is one of frank admiration for the courage, tact, and resourcefulness of most of them—yes, and even for their frequently demonstrated athletic prowess. Contrary to general opinion among laymen, he believes that really brutal attendants are comparatively few. He reminds us that even in the violent wards no attendant is allowed to carry any sort of weapon. All attendants, he claims, must be adroit in ju-jitsu or something like it, and must not hesitate to use their hands when necessary to stop a fight, restore order, or secure obedience to some important command; but the author states that he has never seen a patient injured beyond a few minor cuts and bruises. He approves of the straight jacket, contending that it is not an instrument of torture, as frequently supposed, but a humane device for protecting an excited patient from injuring himself. He approves of the barred windows for himself as well as others. He points out, however, that many factors in institutional routine are well calculated to drive a sane man insane, and that some persons who are fairly sane when first committed eventually crack under the strain. He pleads especially for those who are neither quite normal nor really insane. He says, "They should not be locked in with lunatics and subjected to the same rigorous discipline and unending, mind-destroying repression." Under this discipline, many mild cases eventually become violent.

The author certainly offers no encouragement to sane criminals who feign insanity to escape prison. Hospital attendants, physicians, and patients are aware of the imposture. The criminals are despised and ostracized, and are as closely confined and rigorously disciplined as though they had gone to prison where they belong; but the imposture is entirely too successful with their friends and families, who after they have once been pronounced insane, never again believe them rational and are quick to have them recommitted. The end is likely to be life imprisonment in the hospital, often with real insanity eventually developed.

There is no index.

West Springfield, Mass. MABEL F. MARTIN

The Art of Educational Research: Its Problems and Procedures. By HAROLD H. ABELSON. Yonkers-on-Hudson, N.Y., World Book Co., 1933. Pp. xi, 332.

This book is the most recent of about a dozen that have appeared on methods of educational research, and it will, in the opinion of the reviewer, prove the most satisfactory.

Three of the chapters of the book give a concise summary of statistical principles, including an excellent discussion of the problems that result from the general absence, in educational measurements, of true zeros and equality of measuring units. The first two chapters of the book discuss the place of research in education and the question of how to find and how to evaluate research problems. How to report research is discussed only in the brief final chapter. The major portion of the book deals with the techniques of planning and conducting educational research that will be sound and significant. One contribution of this discussion is its re-organization of research techniques under the five heads of integrative, descriptive, causal, evaluative, and constructive methods. A few changes in the more detailed classification of research methods would seem to have been advisable, such as the inclusion of the case study method under the "inferred control causal method" rather than under descriptive method, and such as giving to testing methods the more distinctive place which they would seem to merit; but as a whole the material is very clearly and thoughtfully outlined.

Throughout, Abelson's book is well organized, clear, and concise. No quotations are made from other authors, although the author draws heavily upon them, particularly upon McCall. Relatively little concrete illustrative material is included in the book, but what is included is used very effectively. This book has made progress towards minimizing merely common sense advice on methods of work, and toward emphasizing, instead, the more technically derived methodological principles. The principal suggestion of the reviewer, however, is that the author should sometime revise this book with both of these tendencies more rigorously developed. In the latter direction, for instance, such minor points as the distinction between chance and systematic errors should be clearly brought out; the whole matter of factor analysis should be given a much more adequate treatment; and in the discussion of studies of problem children, the relatively particular methods of such workers as Samuel Hartwell should be described. There are really quite a few of such general methodological principles which, though admittedly not as technical in their derivation as statistical formulae, nevertheless have been made possible only through the accumulated experience of years of actual research. Such material should furnish the main content for such a book as this.

Psychologists may find this book of considerable value. No corresponding work on psychological research is available, and perhaps there is no occasion for one, since with a few changes of illustrative material this book applies directly to the problems of psychological research.

University of Arkansas ROBERT LEEPER

Le choc des patriotismes: Les sentiments collectifs et la morale entre nations. By EDMOND PRIVAT. Paris, Félix Alcan, 1931. Pp. vii, 179.

The author of this book—not a psychologist—has made contributions to recent political history, and has had considerable first hand experience at observing conflicts among national sentiments, attitudes, and points of view in the League of Nations at Geneva, with which he has been connected. The book is written in a sententious style, abounding in epigrammatic statements. It cannot be called a scientific work.

Pointing out that all centuries have had their conflicts, this Swiss observer holds

that the conflict of the present century is between separatism and federation. Since the economic and political aspects of this conflict have been abundantly investigated, Privat proposes to treat certain different aspects which also cannot be neglected—traditions, customs, fixed ideas, and collective emotions. It has been supposed that it was sufficient to "change the heart of man to bring about definite peace," but history proves otherwise. One must still enlighten the people and adapt their institutions; otherwise their heroic virtues only serve collective passions. The habits acquired carry away the heart, and sentiments attach themselves to institutions (p. vii). Privat points out, as others have done, that the morality of nations is low as compared with that of individuals, this being due chiefly to the fact that the ordinary citizen will not accept criticism of his own country, and that emotional appeals to groups is so effective against rational understanding. National patriotism comes to be the supreme religion because one may even be required to sacrifice his life for it; that for which one pays the most becomes most appreciated.

Privat gives a good account of the aims of the Nationalist movement in India as one of the great experiments now going on in national groups. The Gandhi movement is attempting to raise India to an entirely new national morality; there is developing a patriotism without anything of the warlike spirit. A non-resistance by people willing to make any sort of sacrifice, even to their own lives, is appearing. Gandhi is preoccupied with the soul of his country and is trying to make India heroic but non-military. The leaders of this movement are trying not to humiliate the adversary but to force acceptance of equality. "The Russian revolution attracts attention by its technical experiment; the revolution in India, by its moral experiment. Indeed there is novelty of technique in both. What is new in U.S.S.R. is the end; in India it is the means" (p. 85).

Privat contends that the trouble with the League of Nations is that it lacks the sentimental background and the mysticism which inspire the loyalty found in real national groups and in religions. He rightly recognizes the great rôle of the language factor in such irrational organizations, and sees now in Esperanto the intensive cultivation of the prerequisite basis for the development of trans-national community feeling. It appears somewhat doubtful to the reviewer whether the masses in different national groups can be sufficiently influenced by such an artificially created language to respond with the 'natural' sentiments which bind them to their church and their national groups.

The book, even though its actual contributions to social psychology as a science are small, offers useful data and suggestions to psychologists. It is interesting to note that the erroneous concepts of 'imitation' and 'suggestion,' so frequently found in the old social psychology books, are absent in this treatment. There is no index.

J. P.

Sinn and Unsinn. By WALTER BLUMENFELD. Munich, Ernst Reinhard, 1932. Pp. 110.

During the last two decades psychologists have spent much time discussing the "meaning of meaning," but with little reference to the correlative concept of the meaningless, or the domain of non-meaning. Yet essential formal characteristics of concepts or objects may remain unknown through a failure to delineate properly their opposites and contraries. Sense can only be fully evaluated in the

light of a clear conception of the nature of nonsense. With this fact in mind Dr. Blumenfeld offers a compact yet comprehensive analysis of meaning and meaninglessness, sense and nonsense, from the varied standpoints of philology, logic, metaphysics, and psychology, the net result being a fairly convincing proof of the relativity of the terms and of their dependence on what might be called the functional context in which meaning or its correlative absence or denial appear. Thus the meaning of a term or series may be present or absent according to the circle of conceptional relationships within which the thinker moves at the moment. An item of consciousness may possess semantic and lack purposive or logical meaning, and vice versa. The author discusses five types of meaning and correlative deprivation, viz., semantic, purposive or teleological, configurational (*Eidischer Sinn*), logical, and motivational, with their varying relational criteria from the point of view of formal logic. The meaningless is characterized by the absence, inversion, or perverted use of relationships. It is interesting how according to the author's premises, the 'absolute' of certain idealistic philosophies, as well as solipsism, must inevitably culminate in the acceptance of the meaningless or irrational elements intuitively advanced by mystics from a psychological standpoint. The production of pure or relatively pure nonsense, the deliberate achievement of the meaningless, must be regarded as a mark of high intelligence since it involves the conscious perception of the meanings to be avoided, and this fact may be used as the basis for a new type of intelligence test. The nature of mental processes in the insane, and the course of mental decay, are illuminated by analysis of the types of meaningless activity and thought displayed in such cases. Most of the book is devoted to verities of meaning but a short section discussing the laws and processes governing the apprehending and production of meaning and nonsense will be of particular interest to the psychologist. A still briefer section on the significance of the meaningless in the daily life of the community and especially in relation to laughter, where a whole category of the ludicrous is based on the artistic use of nonsense, completes this remarkably lucid presentation of a difficult subject.

University of Cincinnati CHARLES M. DISERENS

Psychology Applied. By GEORGE W. CRANE. Chicago, Northwestern Univ. Press, 1932. Pp. xii, 586.

If a good textbook is one which is interesting and readable, which requires no mental labor, and which will appeal to second and third rate undergraduate minds, this is an excellent book. According to the publisher's statements, within six months it had been adopted in over 50 colleges and universities. Commercially it is a success.

Nor is this popular approval difficult to explain. The author has used skillfully the tricks of the applied psychology he is presenting. Snappy headings, concrete and vivid illustrations, amusing anecdotes, allusions to sex, omission of everything scholarly (critical evaluations of evidence, problems of method, alternative interpretations)—and the feat is accomplished. Applied psychology is synonymous with common sense plus clever salesmanship. It is a quasi-scientific description of how to "put things over," how to reach the top in a "go-getter" world. The volume might well be dedicated to the spirit of Glenn Frank's approvingly quoted statement: "I suspect that the most important man in the world is the good salesman."

Applied psychology is in constant need of rescue. Unceasing effort is required to keep it above the level of off-hand opinion, charlatanism, preaching, and "success" literature. If it is a legitimate branch of academic psychology it must in some degree be critical, systematic, scientific in spirit, insistent on rigid methods and on thorough evaluation of evidence. Measured by these standards, Crane's book is sadly lacking. It gives only a superficial, incomplete, largely non-psychological, and often lop-sided picture of the topics treated.

To be sure the volume contains many fragments that are amusing, useful, and even true. Interesting thought-provoking discussions occur; for example, on personality differences between the sexes, on the emotional effects of music, on mental development in children, on emotional maladjustments. Likewise in the treatment of advertising and selling, learning, tests, public speaking, education—in fact throughout—one may find sound statements, perhaps whole sections. But the good and poor, the fact and opinion, are so intermingled, and all with sufficient lack of organization and perspective, that one must be very optimistic to believe that students will profit intellectually from their study of Crane's *Psychology Applied.*

University of Chicago ARTHUR W. KORNHAUSER

Intelligence: Its Manifestations and Measurement. By PAUL L. BOYNTON. New York, D. Appleton & Co., 1933. Pp. xi, 466.

This textbook should make a place for itself because of its excellence for training students in the cautions to be observed when interpreting test results. It does this together with offering careful directions for giving tests and an adequate background of the history of intelligence tests and their applications to school and social problems. By taking at times a frankly critical attitude and selecting for discussion studies which bear upon practical problems the author cultivates good sense in the use of test measurements without losing an appreciation of their value. The book provides a training guide rather than a complete compendium. It thus makes a distinct contribution to the series of excellent books on mental tests.

In his discussion of the nature of intelligence, Boynton adopts a social point of view. Accepting social inadequacy as a criterion for feeble-mindedness, he uses "social sufficiency" as the criterion for judging all grades of intelligence. The concept is amplified, however, to mean the inherited capacity of an individual manifested through ability not only to adapt himself to his social environment, but also to reconstruct that environment to meet the fundamental needs of himself and his group. This social criterion tends to change the emphasis from an intelligence-test record to its interpretation in the light of the individual's whole personal history. The author treats in a practical way the causes and control of mental abnormalities, the distinction between deficiency and mental diseases, legal aspects of diagnosis, and provision for gifted children.

A feature which will stimulate much interest among students is the author's frequent use of cases observed and studies made during his own rich experience in dealing with school children, college students, delinquents, and inmates of institutions, while formerly in the psychology department at the University of Kentucky and now in that of educational psychology at Peabody. The text also includes elementary statistical information necessary for the introductory student to understand test data, including simple correlation and the use of the author's correlation chart.

The last hundred pages are a summary of the applications of intelligence measurements to educational problems, racial comparisons, to defectives and delinquents, and to occupational problems. The author does well to recognize that the present tests only indirectly and quite inadequately measure the capacity for social sufficiency which, he believes, should be the aim of capacity diagnosis.

University of Kentucky J. B. MINER

The Psychology of Laughter. A Study in Social Adaptation. By RALPH PIDDINGTON. London, Figurehead, 1933. Pp. 227.

The author is more to be congratulated for the completeness with which he reviews and criticizes the theories of other psychologists and philosophers (from Plato to Kimmins) than for the cogency of his arguments in support of his own theory.

When he contends that "we must seek a *biological* rather than sociological explanation for elementary laughter" (p. 73) because laughter has been observed in apes, we wonder why he should ignore recent reports of the rather highly developed social character of apes, until we learn from a footnote (p. 78) that *social* life "differs from a merely gregarious existence in being associated with culture (in the anthropological sense)." Certainly most biologists apply the word *social* in a less restricted sense.

The author finds his so-called biological explanation in the fact that laughter notifies the mother of the well-being of her infant, just as crying tells her that something is wrong with the baby. "Laughter expresses, maintains, and communicates a mood in which there is no need felt for the organism to make any further adjustment to its environment beyond the one at the moment existing." This is made to explain also the laughter of somewhat older children, which occurs chiefly in those play situations that exercise the 'instinctive' activities of pugnacity and escape—activities that sometimes change suddenly from play to a dangerous struggle. Laughter in such situations show the playmate that the struggle is still "all in fun" and that he may safely continue it. Cessation of laughter shows him that the play mood is gone and he had better stop.

It is with great difficulty that the author bridges the gap between childish laughter in such situations and the laughter of adults at "the ludicrous." He tries to show that the ludicrous, whether pun, practical joke, or comedy, always involves conflicting social evaluations. The discovery that in a given situation two or more equally applicable social evaluations conflict irreconcilably might seem to demand immediate reorganization of the whole scheme of social values; but to laugh is to deny the seriousness of the inconsistency. Thus laughter at the ludicrous serves to preserve social evaluations by expressing complete satisfaction with things as they are. The author does not tell us how his theory accounts for the bitter laugh of disillusionment.

West Springfield, Mass. M. F. MARTIN

A Series of Experiments on the Learning of French Vocabulary. By LOUISE C. SEIBERT. Johns Hopkins University Stud. in Educ., No. 18. Baltimore, Johns Hopkins Press, 1932. Pp. xi, 106.

The problems and methods of this study seem to have grown out of the practical needs of a teacher of French, unfamiliar at first with certain psychological techniques which she finally discovered and improved. There seems to have been good coöperation on the part of 81 college girls who served as subjects. The work covers

only the learning of vocabulary. The author holds, rightly, that results obtained by experiments on nonsense material can hardly be taken over uncritically by teachers of foreign languages.

Four hundred words, suitable in comparative difficulty and origin were selected. From these words, series of 12 words each were made up and put with their respective English equivalents. The learning was always "active," *i.e.* the reproduction (in writing) of the foreign equivalents was always required when the English words were written on the board by *E*. Recalls occurred generally after periods of 50 min., 2, 10, and 40 days; and relearning was required after each recall test, always taking place in the same manner as in the original learning.

Detailed results are given in the tables in the form of comparative means and their standard deviations. We can here indicate only their general trends: studying aloud was found to be more efficient than studying silently; words studied in associated pairs were better learned than when studied in sentences (context); the whole method of studying associated pairs was better than the part method (but the series were short); distributed practice over 10 days was more effective than equal practice concentrated at one sitting; the recitation method of learning was superior to the reading method; the quick learners retained better than the slow learners; those *Ss* that were better in immediate (50 min.) recall were also better in delayed recall; etc. One might quibble over certain details and interpretations, but space forbids. The book is well documented, and a bibliography of 5 pages is added. Unfortunately dates are frequently omitted in references, and no general form is adhered to, even in the bibliography.

J. P.

Dementia Praecox: A Psychological Study. By HARRIET BABCOCK. New York, Science Press, 1933. Pp. 167.

Ultimately the test data from studies such as this should be of distinct value to both psychology and medicine. It is unfortunate, however, that so much that is indefinite, controversial, or completely disproved in psychology should be advanced as verity to those not intimately associated with the science.

One hundred twenty-four "coöperative" praecox subjects made higher relative scores (compared with 216 non-psychotic subjects) on vocabulary than they did on learning tests and on a composite of tests other than vocabulary. That these findings are similar to those reported by J. H. McFadden for 507 feebleminded (*Ment. Meas. Monog.*, 1931, (no. 7), 1-85) is either overlooked or ignored by the author.

The author states that native mental capacity is isolated by intelligence tests (p. 14). "Practical work shows that vocabulary is a better measure of original capacity than school history" (p. 22). Claim is made to having proved previously that the difference between mental ages on vocabulary and on tests other than vocabulary (listed above) is a measure of mental efficiency (p. 15). "The discrepancy between untimed responses which require old associations of a kind which correlate highly with intellectual level [avowedly, vocabulary] and responses which emphasize time and the fixation of new data" (tests other than vocabulary) is given as proof of what the author once claimed was "obvious" (cf. her study in *Arch. Psychol.*, 18, 1930, (no. 117), 58).

While appreciating the difficulties encountered in such a study, the reviewer sees

no justification for the finality of opinion expressed. Emphatic assertion as to what tests measure is not acceptable proof.

Studies in this field would do well to give more attention to factors too frequently ignored. Tests used should have adequate accuracy at the extremes, a patent weakness of most of the measures now used. Any study of dementia praecox should safeguard diagnosis more adequately against the inclusion of other types of dementia as well as against the inclusion of the non-psychotic. In so far as possible, mental ability or abilities, age, and possibly other factors should be held constant. Finally, the basing of any evaluation of pre-psychotic ability on observation made after the onset of the psychosis is, to say the least, a hazardous undertaking.

University of Louisville MILTON B. JENSEN

Personality, Many in One: An Essay in Individual Psychology. By JAMES W. BRIDGES. Stratford Co., 1932. Pp. 215.

The book begins with the following definition: "Personality is that which characterizes, constitutes and distinguishes a human being. It is the totality of all physical and mental characteristic, traits and processes, and their interrelationships. . . . The psychology of personality is therefore the whole of human psychology." What a task to set one's self in two hundred pages!

The author bravely sets out by enumerating the various classifications of personality that have been made on the basis of physique, intellect, temperament, and character, doing it all in such an extremely impartial and abstract manner that the reader begins to wonder, at about page 119, what it is all about. Then, after a long chapter on the "total personality" in which various tendencies toward disintegration are discussed, he takes up the various psychological types as expounded by William James (tender-minded and tough-minded), by Furneaux Jordon (reflective, more-impassioned and active, less-impassioned), by Carl G. Jung (extravert and introvert), by Eduard Spranger (theoretic, economic, aesthetic, social, political, and religious), by the ordinary psychiatrist (manic-depressive, schizophrenic, hysterical, paranoid, psychasthenic, neurasthenic, and epileptic), by Theophrastus (objective observers and subjective depictors), and by various other individuals (classic and romantic types, sensitive and active types, conservative and radical types, egoists and altruists, masculine and feminine types, and, finally, snobs).

The final chapter points out the importance of education in the development of personality and discusses three types as determined by development; the undeveloped, the overcompensated, and the unfolded types.

Northwestern University JOHN J. B. MORGAN

Sweeping the Cobwebs. By LILLIEN MARTIN and CLARE DE GRACKY. New York, Macmillan Co., 1933. Pp. viii, 181.

This book is a sequel to *Salvaging Old Age,* 1930, by the same authors. The book was written primarily to answer the "many hundreds of letters asking for further elucidation of the work done" in the Old Age Center, as described in the former book. The number of letters "is increasing rather than decreasing" and the book is to relieve the burden of responding individually to these letters in an adequate manner. The senior author is thus involved in her interesting and useful work at the age of eighty years.

The authors relate the procedures followed in receptions to the Old Age Center, the getting of life histories—information chiefly regarding the individual's motivations and main lines of activity, tendencies to fall into fixed systems of acting and thinking, reaction patterns acquired and resistances to modification—the purpose being to reveal to the consulter "what he has that is worthy of preservation, what needs correction, and what must be actually banished to bring about a more satisfactory existence for himself" (p. 10). These data, added to those obtained from physical examination and a brief mental test, to determine "how his mind is functioning," supply the basis for the making of daily schedules for different sorts of training, for rest periods and for recreation. The schedules when filled out are carefully checked as to the carefulness of execution. Results obtained constantly serve as basis for further helpful analyses and assignments in the process of "sweeping the cobwebs." Details like budgeting and methods of personal finance; getting the proper tempo for efficiency; maintaining calmness of emotions, progressive attitudes in recreation, personal habits, and contacts with social life are not forgotten in the organization of the consulter's life toward individual freedom and happiness. Such a program would evidently be valuable to most people much earlier in life.

The author's work goes beyond the programs thus briefly indicated: it extends to effecting provisions by society for the better care of the aged. The authors are thus becoming pioneers in a unique sort of social work of high value, that of increasing the worth and the happiness of human life through old age. There is no index.

J. P.

La determinazione del presente psichicho. By RENATA CALABRESI. Firenze, R. Bemporad & Figlio, 1930. Pp. xii, 188.

The nature of the psychological present representing as it does the minimal unit of lived experience, interests psychologists and philosophers in the same way that physical ultimates interest the physicist or mathematician. The present monograph, inspired by Bonaventura's studies of the psychology of time, shows a thorough acquaintance with previous philosophical speculation and experimental studies in this field. In her summaries and conclusions the author displays great ability in drawing logical distinctions and in criticizing the methodology of her predecessors. Learning activity is selected as the best psychic material available because of the simplicity attainable and the possibility of repetition. One set of experiments was carried out by means of Wundt's tachistoscope and is closely related to the work of Wundt and his pupils. The object of these experiments was the determination of the content of the psychological present, the results corresponding closely with those of previous studies of attention, the extension of consciousness, and learning through simultaneous presentation. Another set of experiments involved the use of a new double tachistoscope designed by Bonaventura, and show the limits of the temporal extension in which the results of successive presentations assume for introspection the form of a unified experience. The psychological present is a concrete experience, with limits varying according to the subject and the nature of the content of his experience. In general it may be said, that successive presentations between the limits of 800 and 1000σ are synthesized in cognition and attention to form a single moment of experience. The experiments point to a temporal rhythm inherent in cognitive function, a

rhythm in which the unity of the conscious act is identified with the temporal extension of the psychologically actual and the duration which is directly sensed. The distinction between reality and unreality is illuminated by this comprehensive study, which merits equal praise for experimental skill and philosophical suggestiveness.

University of Cincinnati CHARLES M. DISERENS

Nervous Breakdown: Its Cause and Cure. By W. B. WOLFE. New York, Farrar & Rinehart, 1933. Pp. xv, 240.

This interesting book does not attempt to make any new contributions either to scientific psychology or to technical psychiatry. It aims primarily to help practicing physicians, the patient's family and friends, and above all, the patient himself, to understand the unconscious mental mechanisms concerned in a nervous breakdown. The point of view is that of Adlerian individual psychology, but the illustrations and detailed explanations are largely new, based on the author's own broad clinical experience, many patients having consented to permit the use of their own life stories, disguised to insure anonymity. The author endeavors to convince every neurotic that the intelligent thing for him to do is to consult a psychiatrist who will help him discover the meaning of his symptoms and substitute wholesome attainable goals for the neurotic goals that must be replaced. Illustrative case histories in this book include a college boy who attempted suicide when threatened with failure, a young woman whose ignorance of sex led to nervous breakdown after marriage, a passé vamp whose life-long pride had been her sex appeal and power to enslave men and who found loss of her youthful beauty unendurable, and numerous other examples of unfortunate individuals whose self-esteem depended upon some superficial fancied superiority beyond their own control.

The author contends that any individual whose self-esteem depends on wealth, personal beauty, intellectual preëminence, or any other real or fancied superiority beyond his own control is liable to lose "face," and in consequence, to suffer a nervous breakdown. The only secure basis of self-esteem is service to one's fellowmen. The last two chapters are devoted to constructive advice to patients on the art of widening their horizons and developing creative self-expression.

West Springfield, Mass. M. F. MARTIN

Die seelische Eingliederung in die Gesellschaft. By W. STEINBERG. München, Ernst Reinhardt, 1933. Pp. 126.

Though rich in detail, the theme of the book is clear and simple. Man's social responses are expressions of the inmost core of his being. He *demands* sociality and passionately opens his mind and heart to social influences. To say that man's mind and heart are "molded by social influences" is true enough, but this is not the compulsion which the sculptor exercises upon an inert block of matter; rather, it is the expression of social forces which are demanded, assimilated, and used by the inner self. We speak of the constraint which custom and tabu exercise upon the savage, forgetting that it is because he strives to immerse himself in the group ethos that these customs and tabus are supreme. They are, to be sure, coercive, but the coercion is self-imposed. In modern Europe a doctrine which (like Locke's) has man conform to social usage for the sake of the good opinion of his fellows overlooks the fact that such a desire can exist only because a deeper need for fellowship is already there.

Individual men could never be made into cohesive communities by virtue of pressure acting upon each man *from without.*

The book is didactic and expository. The author aims to "show" how these things are. That his hypotheses permit of, or call for, testing seems not to occur to him. At any rate he betrays no familiarity with the rich source material from child psychology and psychiatry which could be used as a criterion for evaluation of his thesis. For example, though on almost every page one is reminded of the pertinence of Piaget's data on the moral judgment of the child and of psychoanalytic material on the "identification" of the child with the parent, he seems unaware that such studies exist. Greater concern for evidence might contribute more solid and convincing substance to the author's subtle, well-wrought, and highly suggestive doctrine.

Columbia University GARDNER MURPHY

Consciousness: Brain-Child. By PERCY A. CAMPBELL. East Cleveland, Ohio, 1933. Pp. 109.

This slender, theoretical volume, copyrighted and published by the author, reiterates the familiar mechanistic dogma that consciousness is a by-product of brain functioning. Many a discarded theory is dug up to be confuted again, many an old battle refought, many a man-of-straw set up to be knocked down. New metaphors dress up old theories with a semblance of novelty which is enhanced by five neologisms—proud coinages of the author to express his amazingly naïve psychophysical assumptions: (1) *ideopulse,* "the pulse through the brain constituting an idea;" (2) *ideocircuit,* "the circuit through the brain made use of by this activity-pulse;" (3) *ideoprocess,* "the process constituting one item or term only of an idea;" (4) *ideopattern,* "the pattern or brain-trace made use of, or underlying, this process;" and (5) *ideorapport,* "the condition of accordance, correspondence or sympathetic relation existing in any one of these units—with respect to something in the world with which, for the purposes of consciousness, it forms some sort of a liaison. The word *idea* is here used as a generic one signifying any type of consciousness whatsoever; and an idea is here freely considered to be a thing of the brain, wholly cerebral." Precise anatomical and physiological details of neurology are lacking, but some of the broad generalizations which often appear in elementary textbooks of psychology are even more broadly and more dogmatically stated. In so far as the author draws any distinction between obvious facts of introspection and questionable neurological hypotheses, it is to prove the facts by copious arguments and by abundant quotations from philosophers, poets, and psychologists of an earlier generation, and then to state neurological theories as though they were unquestioned axioms.

West Springfield, Mass. M. F. MARTIN

An Experimental Study of Certain Factors Affecting Transfer in Arithmetic. By JAMES ROBERT OVERMAN. Baltimore, Warwick & York, 1931. Pp. vii, 235.

Transfer of training is defined in this study in Ruger's terms "to include the effect of any given experience on any subsequent one" (p. 53). Employing this definition, Overman selected 112 paired "quartets" of second-grade children and studied transfer in connection with the learning of simple types of addition and subtraction. His findings indicate that transfer was an actuality in the case of 99% of the pupils, that the transfer was almost 100% in many cases, and that the amount

of transfer varied with the intelligence of the learner, with the instructional technique employed, and with the closeness in kind of the new to the old material, but not with the sex of the learner.

The special merits of the study lie, not so much in the findings summarized above (though they have large importance for the direction of education, if not for the theory of transfer), as in the extraordinary care with which the study was organized and prosecuted, the quality of the investigator's insight into his problem, the detail of his report, the checking of experimental errors, the testing of hypotheses and assumptions, and the penetrating and intelligent interpretation of the data.

Duke University WILLIAM A. BROWNELL

How We Think: A Restatement of the Relation of Reflective Thinking to the Educative Process. By JOHN DEWEY. 2d ed., enlarged. New York, D. C. Heath & Co., 1933. Pp. vi, 301.

The present book retains the basic ideas set forth in the first edition. A considerable amount of illustrative material has, however, been added, and the position of several chapters has been altered. Part I, which is devoted to the problem of training thought, remains essentially unchanged. The most extensive revisions occur in Part II, which aims to give a theoretical account of the operations of the mental processes as they actually take place in the acquisition of knowledge, in contrast to the older formalistic conceptions. Part III, which deals with the practical problem of training thought, has received a somewhat fuller treatment than in the former edition, although no changes of fundamental importance occur in this section. The present volume contains many valuable pedagogical suggestions drawn from the author's wide experience. The rather free use of a terminology adapted to the level of the average grade teacher (*i.e.* non-professional psychologist) frequently results, however, in mere verbalization and psychological inaccuracy.

Vanderbilt University EUGENE G. BUGG

Phyloanalysis. By WILLIAM GALT. London, Kegan Paul, Trench, Trubner & Co., 1933. Pp. 151.

Phyloanalysis is here presented as something which cannot be expressed in words; it is thalamic rather than cerebro-cortical; it is of the realm of feeling and emotion rather than of concept. There is much about experimental work and laboratory techniques, but the most commonplace principles of scientific method and of laboratory control in experimentation are ignored. Familiar psychological terms appear in a bewilderingly loose usage (feeling-sensation, social image, affective social image, subjective affect-tone, subjective contrast-tone, sensation of anger, feeling of anger, sensation of love, sensation of hate, emotions of love and of hate, feeling-perception). All this vagueness seems to Trigant Burrow, founder of phyloanalysis and author of a preface to this book, exactly as it should be. But to the reviewer the whole thing seems to be no more than an adaptation of psychoanalysis for group work, with most of the scientific crudities of psychoanalysis retained. Much of what is claimed for phyloanalysis has long been the common property of psychology and general psychotherapeutics. Such deliberate efforts to be vague and esoteric are far more likely to confuse and obfuscate than to expand and clarify human knowledge.

University of Oregon EDMUND S. CONKLIN

The Big Cage. By CLYDE BEATTY with EDWARD ANTHONY. New York, Century Co., 1933. Pp. v, 409.

Mr. Beatty, the well-known animal trainer, relates in this book his personal experiences and the methods used by him in training the "big cats" (lions and tigers, principally) and other ferocious animals for their performances in the circus arena. The different steps taken in training the animals to mount their pedestals and perform their other stunts are described and reasons given for the methods used. Though one notes the popular tendency to anthropomorphize throughout, it is evident that the trial and error method, though not known as such, is the fundamental one employed. The account is delightfully illustrated throughout with occasional episodes in the arena involving mix-ups of lions and tigers (both sexes), which now and then result fatally to one or two animals. Beatty also describes his own escapes from various dangerous circumstances and corrects various erroneous views regarding his methods of handling the animals, denying emphatically the so-called hypnotic-eye effect on the animals.

While the book is not scientific it is an attempt by an expert trainer to state facts, though in a somewhat dramatic form. The book is absorbingly interesting, and it may not be out of place in the library of comparative psychologists. Numerous full page cuts are given. There is no index.

J. P.

The Mystery of Sex through Psychology and Religion. By LESLIE D. WEATHERHEAD assisted by MARIAN GREAVES. Forewords by A. Herbert Gray and J. R. Rees. Epilogue by W. F. Lofthouse. New York, Macmillan Co., 1932. Pp. xxv, 246.

This is an example of the kind of book produced by those who uncritically combine psychology and religion. It is not psychology in the sense that it indicates anything new resulting from research, but does combine with some degrees of success the author's rather moralistic attitude and the views held by Ellis and others. There are chapters dealing with sex education of the young, courtship, marital maladjustment, celibacy, social maladjustments in the sex sphere, and "healthy-mindedness." The author's attitude toward sex is relatively modern, for, although he frowns more than usually on promiscuity, he does have a few good words to say regarding marriage and divorce: liberalism which would not be expected from an English churchman. As a general thing the opinions offered are those having acceptance by the mental hygiene movements of both this country and England and by the authors of a rather extraneous group of arguments in the literature of the church. The author treats perversions and sexual maladjustments with much seriousness. There are brief appendices on the physiology of sex which are fairly adequate. All of the psychology in this book is adopted from the descriptive literature and therefore the psychologist would find reference to the original sources of more value. The forewords and epilogue add nothing.

Institute for Juvenile Research, Chicago, Ill. LOWELL S. SELLING

Neuropathology: The Anatomical Foundation of Nervous Diseases. By WALTER FREEMAN. Philadelphia, W. B. Saunders Co., 1933. Pp. 349.

The book is highly technical and written for the advanced student of neurology or pathology. A minute analysis is made of cell structure and composition, or nerve fiber and its sheath, of supporting cells, and of connecting tissue and lymphatic

spaces. The changes brought about in nerve structure and architecture by diseases that include the nervous system in their ravages are set out in great detail. The degenerative alterations in brain and spinal cord are well illustrated by photographs. Great care has been taken to make the work complete, but it is difficult to read without a medical dictionary. The chapters on Intoxications, the Functional Psychoses, and Degenerations give ample evidence of a physiological basis for abnormal behavior. The entire content is the factual results of scientific research.

George Peabody College H. W. FENKER

The Sciences of Man in the Making: An Orientation Book. By EDWIN A. KIRKPATRICK. New York, Harcourt, Brace & Co., 1932. Pp. xv, 396.

The volume gives a description of scientific methods and their applications to the study of human nature and behavior. After an introductory discussion of scientific method, the author plunges into an exposition of the social sciences from anthropology to morals and ethics. Not only are the essential facts and methods outlined, but unsolved problems are clearly indicated and suggestions given for further work. In covering so wide a field of knowledge, the author makes no claims to being exhaustive. Some of his interpretations necessarily tend to be arbitrary and some descriptions of applications sketchy. Very helpful to the student are the illustrative "selected researches" and lists of readings following each chapter.

University of Buffalo HERMAN J. P. SCHUBERT

Über Psychoanalyse und Individualpsychologie. By JOSEF DONAT. Innsbruck, F. Rauch., 1932. Pp. vii, 303.

The author proposes to give a critical review of psychoanalysis and individual psychology from the psychological point of view. Actually, however, his criticism is determined by his philosophical and religious beliefs; namely, by orthodox catholicism. The theories of psychoanalysis and individual psychology are rejected if only for the reason that they are an outcome of "biological materialistic thinking" and because of their immorality.

In aiming at disproving these theories the author collects the arguments from different non-psychological sources: religious, political, and educational writings. The purely psychological arguments are not numerous and are of no theoretical significance. Though written very vividly and giving a clear and simple exposition of Freudian and Adlerian theories, the book is to be considered too much biased to be a good introduction to the study of these theories.

Worcester State Hospital, Worcester, Mass. T. DEMBO

The Story of Scientific Psychology. By ADELBERT FORD. New York, Sears Publishing Co., 1932. Pp. xii, 307.

This book was written to place before the layman the story of the development of psychology as a science. It is composed of a mixture of material, some of which is historical and some of which is material of the type that is ordinarily found in textbooks intended for introductory courses in psychology. Accuracy is frequently sacrificed in an attempt to achieve simplicity and yet in many places ex-

tremely technical words are used with no explanations. Approximately one third of the book is devoted to a discussion of the learning process.

George Peabody College of Teachers — WENDELL W. CRUZE

Courses and Careers. By RALPH P. GALLAGHER. New York and London, Harper & Bros., 1930. Pp. xxi, 404.

This is a vocational guidance text for use in junior and senior high schools. The arrangement is in the form of suggestive topics and questions. The aim is to orient the students of vocational guidance classes as to the nature of generally valuable personality traits; the value of different courses of study; the nature of the many and varied vocations, together with the requirements for those who would succeed in each. There is a tendency toward over-emphasis of special abilities as distinct from interests or aptitudes. The acceptance of current industrial tendencies, rather than any attempt to inculcate sound psychological principles, seems to be at the basis of too much differentiation between men's and women's jobs.

University of North Carolina — HARRY W. CRANE

Validating and Testing Home Mechanics Content. By LOUIS VEST NEWKIRK. Iowa City, University Press, 1931. Pp. 39.

From more than 100 tasks of home mechanics, 72 have been selected on the basis of high favor on the part of schools giving home mechanics courses and according to the social utility of such tasks as indicated by adults. A test of ability to do these tasks has been constructed and two forms are provided for diagnosing practical mechanical ability and for testing achievement. The test was found to correlate $0.26 \pm .09$ with the Stenquist test, $0.44 \pm .07$ with I.Q. obtained from the Otis group test, and $0.64 \pm .06$ with grades given by teachers of home mechanics.

Southwestern College — W. R. ATKINSON

The Major Forms of Inhibition in Man. By ERNEST BURTON SKAGGS. Chicago, University of Chicago Press, 1931. Pp. vii, 104.

The monograph is eclectic and as the author says there is "certain discontinuity between chapters." Gaps in the system of facts in the field of inhibition, he thinks, make this "discontinuity" temporarily inevitable. The book is addressed to students: a source book for their acquisition of facts about inhibition which conveniently spares them a certain amount of search through the psychological and physiological literature.

Bethel, Maine — H. G. BISHOP

Ethics and Moral Tolerance. By ARTHUR KENYON ROGERS. New York, Macmillan Co., 1934. Pp. v, 323.

It is an indisputable fact that ethics somewhere must connect with psychology. Whether moral action is regarded as conformity with an ultimate law or a decision concerning what one ought to do in specific instances, there are psychological problems involved. Professor Rogers follows a psychological approach to ethical questions in preference to the cosmic abstractionism of both the older absolute idealisms and the newer logical rationalisms. The basis of morality he locates in man's affective life and takes as his position an ethics of sentiment.

So far so good. But unfortunately, although Professor Rogers inclines toward a psychological position, he is at the same time interested in the generality of moral judgments and rigidly opposed to the pragmatic doctrine that morality concerns specific situations. For this and other reasons his affective factors—sentiments of approval and disapproval—are really universalized counters in discussion, rather than actual psychological phenomena. So, despite the original agreeable proposition connecting moral behavior with psychological happenings, the present essay falls short of a satisfactory statement of the psychological aspects of moral phenomena.

Indiana University J. R. KANTOR

Methoden der individuellen Auslese für gewerbliche Berufe. By THEOBALD FÜRST. Abderhalden, Handbuch der biologischen Arbeitsmethoden, Abt. IV, Teil 16, Heft 1. Berlin and Vienna, Urban & Schwarzenberg, 1931. Pp. 288.

The methods described in this volume are not so much of interest to the psychologist as to the physician who has to pass on matters of general health and constitution in connection with problems of vocational guidance and who is required to examine large groups of individuals in schools, clinics, special bureaus, or in industry. The author shows that relevant information on hereditary and morphological aspects can be obtained even in investigations of such kind. For more detailed investigations he presents special methods for examining skeleton, muscular system, respiration, heart and circulation, blood, nervous system, skin, eye, and ear. Most of the methods can be employed without the use of elaborate equipment. No doubt, some of the methods recommended by the author, although of interest from the point of view of research, are of slight value when it comes to making practical decisions in questions of vocational guidance, selection, and placement. The book may serve as an introduction to the widely scattered German literature on the 'applied' side of the science of constitution.

University of Chicago HEINRICH KLÜVER

L'écriture et le subconscient: Psychanalyse et graphologie. By PIERRE MENARD. Paris, Felix Alcan, 1931. Pp. 172, 45 autographs, 22 Fig., and 5 diagrams.

Graphology is regarded as a chapter in the psychology of movement, and as a method of the scientific study of the 'subconscious.' The author discusses the field and limitations of graphology as a scientific method, the objections and difficulties, and the techniques of studying and analyzing handwriting. Handwriting, says Dr. Menard, is a graphic record of uncensored gestures by means of which energy, extent, direction, form, and rhythm can be determined and studied in a precise and objective manner. Each of these aspects of handwriting is discussed and analyzed in detail, stress being laid on rhythm as a subject too little considered in psychological investigations. "The whole of the subconscious is recorded in the rhythm of handwriting" (p. 142).

The book appears to the present reviewer to be chiefly a contribution to the techniques of analyzing handwriting. Although much of the interpreted material is taken from diaries, from the context of which the author has taken clues, it is not clear that "sensuality," "anguish," "egoism," and other mental states described as "subconscious emotions" are themselves reliably defined or reliably correlated with their alleged characteristics in handwriting.

University of Iowa HAROLD H. ANDERSON

De la connaissance de soi. LEON BRUNSCHVICG. Paris, Felix Alcan, 1931. Pp. xi, 196.

This series of lectures attempts to construct a philosophy rather than a psychology of the self. The philosophy in which it engages is, however, mainly of that literary variety so exasperating to readers of a scientific bent. If one can bring himself to read it as he would read Hegel's *Phenomenology of Spirit,* one will find that it has a certain charm. The technique of the discussions is purely dialectical. Thus man begins as *homo religiosus,* is complemented by *homo faber,* develops into *homo magicus,* who, feeling the need of other men, becomes *homo loquens,* who generates by his own necessities *homo politicus.* These various selves are of course all "aspects" or "stages" of the 'Self;' all necessary stages, moreover, in the knowledge of the human pirit.

This book is an attempt to write an edifying series of lectures, and in the eyes of people capable of being edified, it will seem to have succeeded.

Johns Hopkins University — GEORGE BOAS

Classroom Organization and Management. By FREDERICK S. BREED. New York, World Book Co., 1933. Pp. xvi, 472.

This book is one of the "Measurement and Adjustment Series," edited by Lewis M. Terman. It is written for the practical school man, not for the professional psychologist for whom it contains nothing new. This statement is not intended as derogatory and the reviewer feels sure that teachers of educational psychology in our colleges and normal schools will find stimulating material on the subject of achievement and intelligence tests in relation to curriculum reorganization, vocational guidance, supervised study, classification of pupils, and similar topics.

The time is rapidly approaching when the school must judge itself by the success of its students vocationally and avocationally sometime after leaving school. Early determination of ability, of special abilities and of their lack, with corresponding adjustments in extent of academic training and emphasis upon trade training and motor skills must be made early and must be reflected in a differentiated curriculum. Dr. Breed gives valuable suggestions along this line from the standpoint of applied psychology.

University of Pittsburgh — W. T. ROOT

Case Studies in the Psychopathology of Crime. By BEN KARPMAN. Washington, D.C., Mimeoform Press, 1933. Pp. x, 1042.

Psychiatrists as well as sociologists are beginning to make assiduous use of the case method in their studies. For the psychiatrist this is undoubtedly a step forward in the right direction, for there is hardly any useful alternative method for the study of abnormal behavior. As long as the conception prevailed that mental disturbances were merely the malfunctioning of an organ—say, the brain—the individual as such could be put into the background, behind biopathological conditions. But that time is happily passing.

In the present voluminous work Dr. Karpman publishes in elaborate detail the case studies of five patients in Howard Hall, the criminal department of St. Elizabeths Hospital. Among the materials, besides the official records, are autobiographies of the subjects, letters, opinions of other prisoners, etc. There is undoubtedly a mine of

material here for the study of the criminal insane. The psychiatric interpretation of the present material Dr. Karpman promises to incorporate in a second volume.

Indiana University. J. R. KANTOR

Elementares Lehrbuch der Sozialpsychologie. By W. HELLPACH. Berlin, Julius Springer, 1933, Pp. v, 165.

The author of this brief outline defines social psychology as the study of the means whereby persons are psychically related. Accordingly, he assumes that there are five general subjects to be investigated: (1) the interaction of persons brought about either directly or indirectly (expression, communication, or action); (2) the bases that make for cohesion or detachedness of people (love, hate, etc.); (3) the character of collectivities on the basis of strength, duration, cohesion, etc.; (4) the influence of a collectivity on the innate romanticism or realism of individuals, whether they become leaders, followers, antisocial, etc.

As the above statement indicates, Hellpach uses the term social psychology as a catch-all for various kinds of phenomena. He does not differentiate between psychosociology (the psychic conditions of society); social psychology (the social or cultural forms of behavior); or interpersonal psychology (the study of reactions of persons to persons). More serious than this failure to make social psychology a distinctive discipline, is the fact that the author does not describe and analyze fundamental psychological mechanisms, despite much discussion of "laws." And finally, we may point out that the entire trend of the book is toward traditional conceptions of psychic states operating directly upon other psychic states or through the intermediation of bodily movements and other actions.

Indiana University J. R. KANTOR

College and Life. By M. E. BENNETT. New York, McGraw-Hill Book Co., 1933. Pp. xiv, 456.

This book is designed to meet an important need, and it meets it well. Its purpose is to furnish rational guidance to the college student, with respect to his inner problems, his social problems, his life-plan, and methods of study. The book may advantageously be placed in the hands of any serious-minded young person about to enter college; and also in the hands of any student who might not be serious-minded: it ought to make him so. It is written without condescension and without exhortation; and its motto might well be: "Come and let us reason together." Its standpoint and statements are based on modern psychological principles and findings. It shows how this new knowledge can be utilized for the benefit of the college student.

Before a second edition of the book is called for, which will probably be the case, the author might look up the quotation, "Manners make the man." It is generally ascribed to William of Wykeham, and not to the source the author mentions. Also, the feebleminded child mentioned on p. 192, who could name the capitals of the states, was something more than an idiot.

Knox College R. T. WILTBANK

An Outline of General Psychology. Rev. ed. By ROBERT H. GAULT and DELTON T. HOWARD. New York, Longmans, Greene & Co., 1933. Pp. vii, 452.

This new edition makes only very slight changes in the first one; a few chapters have been shifted and many have been put under slightly different captions—

"Sensing or Being Aware," for "Sensation," "The Nervous System," for "Reaction and the Nervous System;" "Perceiving," for "Perception," "Motor Reactions—Chiefly Native," for "Instinct," and "Motor Reactions—Chiefly Learned," for "Habit." Neither the methods and results of psychology nor the references are brought up to date. The point of view remains the same. Such terms as 'refractory phase,' the 'all-or-none law,' and such contributors as Lashley, Adrian, and A. Forbes, are not mentioned in the Index; and the questions and chapter-references, in the last part of the book, are practically unchanged. This includes spelling errors and all (except that 'Lickey' has taken on the necessary "1"). The chapter on "Applied Psychology" shows no recent progress, and that on "A Sketch of the History of Psychology" does not even refer to the excellent recent histories by Gardner Murphy and E. G. Boring. The authors may feel that such changes are not necessary for the purposes of the book.

The printers and binders have done an excellent job.

Persons one and Three. By SHEPHERD IVORY FRANZ. New York, Whittlesey House, 1933. Pp. 188. J. P.

This book by Franz was his final contribution to the field of psychology. It is undoubtedly a major contribution in spite of its apparent duplication of similar material extant in the writings of Janet and Prince. No other work sets forth the procedure and technique of reëstablishing a dissociated personality with the same clarity. The pertinent facts relating to the disintegration and reintegration has been woven into an excellent narrative. The reader will be surprised with the tremendous amount of labor expended and the tediousness of such labor. It is recommended to psychologists since in it Franz removes the obscurity which has usually clouded the description of earlier work. Since no preconceived theory on the part of the author had to be proved one finds the discussion free from the mystical concepts of the analysts as well as devoid of other terminology which would tend to hide the real facts. The lay reader will find it intriguing in spite of the incorporation of a considerable amount of conversational material. It will become one of the classic examples of multiple personality and be grouped with such notable cases as Miss Beauchamp, Mr. Hanna, and Patience Worth.

Johns Hopkins University ROY M. DORCUS

Specialization of Verbal Facility at the College Entrance Level. By WARREN G. FINDLEY. Teachers College Contribution to Education, No. 567. Bureau of Publications, Teachers College, Columbia Univ., 1933. Pp. v + 76.

A study of differentiable mental ability is made by applying the Spearman tetrad technique to the results obtained from a scientific and a literary vocabulary test given to 391 college freshmen applicants at Cooper Union Institute of Technology and to 139 students at Yale University. The factor analysis shows that the two tests overlap 86%; 8% of the variance is due to specific factors, and the other 6% is attributable to chance factors of unreliability in the tests; whereupon the author concludes that the two tests measure chiefly a common element and that verbal facility is unitary. The original test data are correlated with the students' records at the two schools and the results suggest a "reliably higher" relationship between scientific vocabulary and grades in the science curriculum than between the literary vocabulary and grades

in the academic curriculum. This, it would seem, is due to the specially constructed scientific test, which is, of course, in favor of the aptitude emphasis of this portion of the study.

The author offers special statistical technique to take care of the inequality between the two experimental groups. These techniques "are designed to do for differences between means what partial correlation does for raw coefficients of correlation." Experimental control would be better. The study shows several tendencies to calculate abilities rather than measure them.

Lincoln Memorial University JESS H. EDDS

Handbook of Child Psychology. Rev. ed. Edited by Carl Murchison. Worcester, Clark University Press, 1933. Pp. 926.

This second edition of the Handbook of Child Psychology retains 15 chapters from the first edition, and includes 9 chapters on new topics. With few exceptions, the chapters retained have been enlarged by the inclusion of references to studies reported since the appearance of the first edition. The volume has increased from 687 to 926 pages and the bibliographies have increased from 1470 to 2815 references. The new topics include discussions of the origin of behavior in pre-natal growth and in development during the first two years; physiological appetites, difficulties of adjustment, and speech pathology; measurement of mental growth and sex differences. A chapter on the adolescent child is also included. These problems have become increasingly important for the student of child psychology and their addition increases the value of the handbook.

Johns Hopkins University BUFORD JOHNSON

Modern Man in Search of a Soul. By C. G. JUNG. Trans. by W. S. DELL and C. F. BAYNES. New York, Harcourt, Brace & Co., 1933. Pp. ix, 282.

This volume consists of 11 more or less related chapters, all save one having been given in lectures. According to Jung, much of the neuroticism which obtains today is attributable to the loss of religious faith. The methods of Freud and Adler are held to be inadequate in dealing with what the author holds to be a spiritual problem, since they fail to take the entire psyche into account.

The present book, although written for the most part in popular rather than scientific style, contains many suggestions of therapeutic value for the student in this field. Furthermore, modern man's need for a more unified self based upon a worthy ideal is stated in a sufficiently convincing manner. The author, however, indulges frequently in a type of *a priori* speculation which though perhaps carrying comforting implications for the religionist is of questionable scientific value. Whereas Freud made the unconscious something to abhor, Jung actually raises it to the status of the World-Ground.

Vanderbilt University EUGENE G. BUGG